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RESPONSE ELIMINATION WITHOUT PERFORMANCE

BY

H. M. B. HURWITZ

From the Department of Psychology, Birkbeck College

Hungry rats were taught to press a lever in order to obtain a small quantity of food. It has been shown that the strength of the lever-pressing response can be reduced by allowing the animals to explore the box without finding food. This result conflicts with a simple reactive-inhibition theory of forgetting and response-elimination. Three possible explanations have been considered: (*a*) that the failure to find food changed the motivational condition of the animal; (*b*) that a conditioned drive, associated with the food-box, underwent extinction; (*c*) that a genuine response extinction occurred, and that this could be accounted for in terms of a principle of reaction-chain acquisition and extinction which differs in important respects from Skinner's (1938) and Hull's (1954) principles of conditioning.

INTRODUCTION

Skinner (1938) maintained in his "law of the extinction of chained reflexes" that only members of a chain actually elicited undergo extinction. Hull (1943, 1953) commits himself to an interference theory of experimental extinction. He asserts that in making a response there is added an increment to a drive state akin to "tissue injury, fatigue or 'pain' (which) tends to inhibit that reaction, i.e. to prevent the occurrence of the response in question and possibly other responses" (1951). The gradual dissipiation of this drive state conforms to the conditions of a reinforcing state of affairs and, in accordance with Postulate 4, Principle of Behavior (1943), should lead to the strengthening of stimulus-response connections. However, since the drive undergoes diminution when the animal is relatively at rest, the response being reinforced is that of being *inactive*. Thus experimental extinction is accounted for in terms of *reactive inhibition* and *counter conditioning*, i.e. of linking stimuli previously related to the making of one set of responses (R_1) to other responses (R_2) essentially anithetical to the first set. In short, in order to obtain the ultimate elimination of a response, it must be elicited for two reasons: (*a*) firstly, to ensure that it will become associated with the "tissue injury, fatigue or 'pain'" drive and (b) secondly, to provide the drive (fatigue from work done).

In his recent book, Deese (1952) reports a few studies which showed, in opposition to the predictions of Skinner and Hull, that a response can be extinguished without its performance. Both Seward and Levy (1949) and Deese (1951) employed hungry rats in a maze to demonstrate this extinction phenomenon. Whilst Seward and Levy used an elevated path leading directly to the goal box, Deese chose a U maze in his experiment. After the experimental group of rats had been well trained in the

maze habit, they were given an opportunity to explore the goal box from which all traces of food had been removed, without being made to run through the maze. Subsequent extinction trials on the maze showed that the running response had become weakened as a consequence of this earlier experience. Control groups gave higher extinction scores than the experimental groups.

In interpreting these experimental results it could be argued that the exploration of the empty goal box involves the extinction of response tendencies based on secondary reward, so that the more rapid decline in response strength during the test trials for the experimental groups is in line with theoretical expectations from Hull's theory. But this interpretation would not hold for the Skinner box. Here the animal is not *consistently* rewarded in the presence of the stimuli associated with the box, because, during initial training there is partial extinction of (*a*) exploratory behaviour, including trough-going responses; (*b*) responses to features of the apparatus which do not lead to reward and (*c*) responses which must ultimately be fashioned into the reflex-chain. Thus only those cues distinctly associated with the arrival and ingestion of food acquire secondary reinforcing property. It could therefore be maintained that the box, *per se*, does not act as a secondary reward, but that this function is delegated to isolated features of the apparatus. If, therefore, the findings of Seward and Deese could be duplicated in a Skinner-box, its explanation would require some other principle than the secondary reward hypothesis.

The purpose of the present experiment was to determine whether the bar-pressing response in a Skinner-box could be partially extinguished without being made by the animal.

Subjects: The subjects were 12, 2–3 month old, male hooded rats drawn from the colony bred and maintained in the animal laboratory of the Psychology Department.

Apparatus: Two modified Skinner boxes were used. The first box served as the *training* apparatus, the second as the experimental situation. The second of these boxes measured 12 in. × 9 in. × 12 in. and was fitted with a special food delivery mechanism (Hurwitz, 1953) which delivered pellets weighing 0·05 gms. The food trough was placed 5 in. to the left of the lever and was shielded by a plastic door which had to be nuzzled open before the food could be retrieved. The lever was mounted 3 in. above the floor. It measured 1 cms. square and required a pressure of 2 gms. to move through ¼ in. The lever could be automatically presented and withdrawn by means of two large solenoids. The box was illuminated by a 60-watt lamp mounted 1 ft. above the copper mesh box-lid. This apparatus was placed into a sound resistant case and covered by double glass panes. A buzzer sounded as long as the lever was held down. This ensured that the very slight noise made by the feeding machine could not be detected. The circuits were arranged in such a way that an animal had to retrieve each pellet before the lever would operate the feeding machine again. Thus it was impossible for the animal to accumulate a pile of pellets by pressing the lever in rapid succession.

The training box differed from the experimental box in the following details: the lever was 1 in. square; the distance between lever and trough was reduced to 3 in.; the trough was smaller; the box was not placed into the sound resistant case, but formed part of the battery of cages in the laboratory; the feeding machine was of different design, and gave a sharp click when activated.

It was possible to set up the lever pressing response in a few minutes, with a minimum of pretraining.

Records were obtained in two ways:

(*a*) Lever and trough responses were cumulatively recorded on two counters. Readings were taken at the end of each experimental phase;

(*b*) Lever and trough responses were recorded on a fast moving tape against a time base. It was therefore possible to have a clear record of the serial order of the events, as well as a record of their temporal relations.

Procedure

For approximately two weeks before the first training session, the animals were placed into a special feeding-fasting-schedule apparatus which automatically made food available once daily for two hours. Three days before the training session proper, the animals were transferred to individual cages, and were given food (one hour) at the time at which they were to be used in the experiment.

Day 1—Training: The subject was placed in the training cage. Some food had been put into the trough. As soon as the available food had been eaten, the experimenter operated the feeding machine 5 times in succession, at 15–20 second intervals. The lever was now strewn with bits of food and the animal was left to discover that lever-pressing issued food into the trough. The animals made 60 unassisted trials, taking from ½–1 hour. At the completion of the training trial the animal was given as much food as it could eat in 1½ hours.

Day 2—On the second day the animal was introduced into the experimental box. A few pellets were placed in the trough. The lever had been withdrawn and was only inserted after the food in the trough had been eaten. The animal was left in the box until 60 70 pellets had been eaten. Again the training session was followed by an extended free eating period.

Day 3—90 pellets had to be eaten in the experimental box.

Day 4—On the final day of the experiment the following conditions were observed: (*a*) three pellets only were placed in the feeding machine. The animal was left in the box until it had made 40 unrewarded lever pressing responses; (*b*) in the second phase of the experimental session, the subjects were randomly assigned to one of three groups:

	Phase a	*Phase b*	*Phase c*
GROUP I	40 L	2 hours in the box, lever available	
GROUP II	40 L	2 hr. 10 m. in home cage	½ hr. in box, lever available
GROUP III	40 L	2 hr. in box, lever withdrawn, 10 m. in home cage	½ hr. in box, lever available

(*c*) In the final phase Group II and III were given a ½ hr. in the box, with the lever available. Group II had been taken out of the box and returned to the home cage at the conclusion of phase (*a*) for 2 hrs. 10 mins.; whereas Group III had remained for a further 2 hrs. in the box after phase (*a*) had been completed. However, the lever had been withdrawn after the 40th unrewarded response. This group was removed from the box for 10 mins. before the final phase of the experiment, and was kept in the home cage in the intervening period.

Results

Table I details the results of this experiment. The results are unmistakably in the direction predicted by a non-response extinction hypothesis. The experimental groups, i.e. Group II and Group III may be compared on the following items by means of the Mann-Whitney, non-parametric U-Test (Mann & Whitney, 1947): (*a*) differences in total number of *lever-pressing responses* (L) occurring during the final phase of the experimental extinction session, (*b*) differences in the total number of trough responses (*T*) made during the final phase of the experimental extinction session; (*c*) differences in the total number of trough-responses made during the first extinction session. As regards the first two tests, $U = O$ and $p = 0{\cdot}05$; when the two groups are compared with respect to the number of trough responses made during the first extinction session, the U-tests showed that the groups are not different, but derive from the same statistical population. Again, this result is in the expected direction, since the training conditions should make for homogeneity of behaviour under identical experimental extinction conditions.

Discussion

The results of this experiment seem to support Seward and Deese's findings. It has been shown that the mere presence of the animal in the empty food box leads to a decrement in the *strength* of the critical response. Since we have ruled out that it is the extinction of secondary reward effects associated with the box which is responsible for this observation, it is necessary to look elsewhere for an explanation.

(a) *Response Strength and Habit Strength.*

So far the results support the suggestion that an instrumental response can be weakened without building up specific reactive-inhibition. Since a weakening of the response could be attributed to a variety of factors, this result, in itself, does not force us to conclude that the response is being extinguished. It is consequently necessary to apply some additional criterion in order to establish whether the response decrement relates to performance ((sEr) in Hull's system of symbols) or to a direct weakening of the habit (sHr) being tested. The classical method of arbitration is to obtain spontaneous recovery scores (Pavlov, 1927; Ellson, 1938). If the response does not recover some of its original strength after a sufficient lapse between the extinction trial and the recovery test, we may conclude that the original experiment was, in fact, successful in breaking down the *habit*. On the other hand, a large recovery indicates that the previous decline in response-strength relates primarily to *performance*, so that little weakening of the habit has taken place.

TABLE I

Group		*Experimental Conditions*	*Total L*	*L in phase (c)*	*Total T*	*T in phase (a)*	*T in phase (b)*	*T in phase (c)*
I	1	2 hours free responding	340	—	248	—	—	—
	2		274	—	234	—	—	—
	3		332	—	283	—	—	—
	4		311	—	239	—	—	—
II	1	40 unrewarded L; followed by 2 hrs. 10 mins. in home cage and a final ½ hr. in apparatus, lever available.	108	68	187	74	—	117
	2		171	131	127	49	—	78
	3		104	64	108	54	—	54
	4		112	72	105	77	—	48
III	1	40 unrewarded L; followed by 2 hrs. in apparatus, 10 mins. in home cage and ½ hr. in apparatus, lever available.	88	48	259	80	136	43
	2		98	58	163	70	58	35
	3		79	39	168	68	72	28
	4		70	30	168	65	73	30

Deese's conclusion that habit extinction has occurred in his experiment is based upon the application of a spontaneous recovery test. The experimental group was divided into two sub-groups. For one sub-group the "first four trials of the usual type of response extinction were give 24 hours after non-response extinction" (1951). It was argued that the difference in the experimental conditions between these two groups allowed for a comparison for spontaneous recovery from non-response extinction to be made. However, since the comparison of scores failed to reveal a difference

between the two groups, and in addition contrasted sharply with the recovery test scores from two control groups, Deese concluded that non-response extinction trials result in a genuine habit-strength decrement of the critical response.

This conclusion can be challenged: the spontaneous recovery test holds only if the positive drive can be assumed constant and there are suggestions that this may not be so in this type of experiment. Thus, some recent experimental work on *conditioned drives* (Zeaman & House, 1950; Danziger, 1951; Calvin *et al*, 1953) as well as Hebb's 1948) suggestive analysis of motivation, indicates that a *situation* which has been associated with a primary drive acquires the property of facilitating instrumental actions already associated with drive reduction. For example, in Danziger's (1951) experiments, two groups of rats were compared with respect to their performance in a straightaway. One group of food-satiated animals, with previous experience of hunger and reward in the maze, performed more vigorously than a control group which had the same acquaintance with the situation, but had never experienced hunger or reward in it. Furthermore, the former group ate when food was offered to them in the goal box, whereas the control group declined it. Calvin *et al* (1953) have recently published some related material: two groups of rats were placed for a short time into a striped box 22 hours and 1 hour respectively after the last feeding session. When later both groups were allowed to eat in the striped box, the 22 hour group ate considerably more than the 1 hour group even though the groups had been made equally hungry. Here we have support for the view that the mere concomittance of a situation and an unsatisfied drive suffices to link the situation with the consummatory behaviour characteristic of the drive.

The importance of these, and other studies, on acquired drives for the analysis of experimental extinction is obvious: it increases the already existing difficulty of distinguishing between deterioration in performance and the breaking down of a habit. On the basis of present evidence, it is reasonable to assume that the Skinner-box has acquired conditioned drive properties. During unrewarded trials, this conditioned drive probably undergoes extinction so that the total available drive strength underlying the performance of the instrumental response declines. Thus in explaining the present results, it could be maintained that the group which was confined to the apparatus for 2 hours without an opportunity of making the instrumental response, underwent a drive-extinction, so that when the lever was presented at a later stage of the experiment, the drive conditions were unfavourable to further responding. In other words, the extinction of the conditioned-drive may serve to account for the statistically significant differences between our two experimental groups. On this interpretation our results do not support the non-response extinction thesis of Deese: it could still be argued that the habit-strength *per se* has not been differentially effected by the difference in experimental treatment of Group I and Group III.

(*b*) *Reaction-chain extinction.*

Our results may be interpreted in yet another way. It could be maintained that Group III which, it will be recalled, remained in the box for 2 hours after the lever had been withdrawn, had ample opportunity for trough-exploration. This should result in the experimental extinction of the trough going habit. Inspection of Table I favours this interpretation: Group III made fewer responses during the final phase of the extinction trials. However, they made an average of 164 trough responses during the 2 hours in the apparatus, so that their overall median score for trough responses is 266. When these results are compared with the number of trough responses made by the control group, it is found that the experimental group made slightly more trough

responses. However, this difference is not statistically significant, so that we conclude that the two groups behaved in a similar manner with regard to this item.

To what extent does the extinction of the later members of a reaction chain affect the extinction of the initial members? The prediction derived from a simple reactive inhibition theory states that it should not lead to a decrement in the habit strength of the initial responses, except by way of generalization. Unfortunately, we lack experimental evidence on this point: the crucial experiment has not yet been performed. But at first glace it seems reasonable to suppose that the extinction of goal or near to goal, reactions should drastically affect the response-strengths of responses which have probably been conditioned on the basis of the *secondary reinforcing function of proprioceptive cues*. Thus, it seems consistent with Spence's (1947) analysis of the goal-gradient phenomenon and its relation to secondary reinforcement, to predict that the extinction of a near to goal reaction undermines the response strength of earlier members of the reaction-chain since secondary reinforcers of the proprioceptive kind will have lost their reinforcing, behaviour sustaining, properties during these extinction trials. It should be noted, however, that this approach conflicts with a rigid reactive inhibition approach, though it is in line with what is known about secondary reinforcement, namely, that responses extinguish more rapidly in the absence of stimuli previously associated with primary reinforcement.

It is therefore possible to interpret our results, as well as those of Seward and Deese, in terms of a reaction-chain extinction principle, which states that the extinction of the initial members of a reaction-chain may be brought about by extinguishing terminal members of that chain. This principle conforms with Pavlov's discovery that conditioned reflexes elaborated on the basis of other conditioned reflexes undergo extinction unless the latter are continually reinforced by the unconditioned stimulus (Pavlov, 1927, p. 33, p. 54). It furthermore raises a new problem: to date there exist a mass of data on the goal gradient, or gradient of excitation. Hull's recent analysis (1954) has shown that goal gradient phenomena are explicable in terms of (*a*) gradients of primary reinforcement, (*b*) secondary reinforcement gradients, and (*c*) stimulus generalization gradients. Comparable data on extinction is not available, perhaps, because it was thought that the principles of habit acquisition apply, *mutatis mutandum*, to experimental extinction, though in fact, there is considerable evidence to the contrary. Nevertheless, it was Hull's belief that experimental extinction phenomena were merely the reverse side of the learning process and consisted of learning not to perform previously acquired reactions.

If our results, and those reported by Seward and Deese are examples of genuine response-extinction without performance, the explanatory value of the reactive inhibition theory of experimental extinction is weakened.

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EXPERIMENTS ON HABIT REVERSAL IN THE WHITE RAT FOLLOWING ELECTRO-CONVULSIVE TREATMENT

BY

MARIANNE FRANKENHAEUSER

From the Psychological Laboratory, University of Stockholm, and the Nobel Institute for Neurophysiology, Karolinska Institutet, Stockholm.

Rats in a T-maze were taught two opposite habits successively, the first being heavily overlearned compared to the second. One group of 14 rats was then given a series of 6 electroconvulsive shocks. The control group of 11 rats underwent a dummy procedure.

Tested subsequently the control group maintained the second habit but among the shocked animals some adopted the second habit and some the first. This was no evidence of habit reversal, however, for the behaviour of the shocked animals at the choice point was not distinguishable statistically from random behaviour.

The data of earlier workers are re-examined and, in all cases, animals said to have undergone habit reversal act as if at random.

I

Introduction

The habit reversal theory is an attempt to account for the therapeutic effect of convulsion therapy in psychiatric disorders. The theory holds that convulsive shock more easily disorganizes recently acquired habits than those acquired earlier. In the case of the patient it is assumed that the old habits correspond to a normal adjustment and the new habits to abnormal behaviour patterns. The treatment weakens the recent acquisitions, and the earlier normal behaviour is reinstated. Experimental support for this theory was offered by Rodnick (1942) from experiments on metrazol-treated schizophrenics. Duncan (1948) claimed that a single electroconvulsive shock produces habit reversal in rats trained on two simple maze habits in succession. Braun and Patton (1950), also utilizing maze learning in rats, concluded that a series of 12 electroconvulsive shocks give rise to habit reversal only provided the second habit in the learning sequence is relatively more difficult.

Duncan's conclusions were not supported by a preliminary experiment by the present author in which one electrical convulsion was given. No habit reversal was seen. The present investigation was planned to give a better chance for habit reversal to manifest itself. The first habit was overlearned for a considerably longer time than the second, so that reversal to the first habit might be facilitated. Furthermore, six shocks were given instead of one. The conditions, it was thought, might also approximate more nearly to the clinical situation. Nevertheless, no evidence of habit reversal was obtained.

II

Experimental Procedure

Male albino rats of the Wistar strain were used. They were maintained on water and a stock diet. (Animals and composition of diet were kindly provided by Professor E. Brunius, the National Institute of Public Health, Stockholm.) At the start of the experiment the rats were 38-44 days old. Two tasks were taught in succession, turning to the right and to the left in a simple T-maze in order to obtain a food reward. A 15-hour hunger drive served as motivation. Before learning began, the animals were given preliminary training in the maze in order to secure adaptation to the experimental situation. First each rat was allowed to explore the maze freely for 6 trials of 3 min. each spread over 3 days. No food was available in the maze at this stage. The next step was to train each animal to eat in the straightaway of the maze by 15 trials spread over 3 days.

Learning period.

The training of the two habits was accomplished in the following manner. First, 12 rats were trained to turn to the right for a food reward (0·2 grams) and 13 rats to turn to the left (Habit I). Then each animal was trained to turn in the opposite direction to reach the goal (Habit II). The criterion of learning was arbitrarlily chosen as 4 correct out of 5 consecutive trials. Ten trials a day were given. The training of habit I was continued until each rat had completed 120 runs after reaching the criterion. Training for habit II continued only until the rats had made 30 runs after reaching the criterion. On the basis of their performance in learning habit II the animals were divided into an experimental group of 14 animals and a control group of 11 animals. (One of the experimental animals accidentally got a subconvulsive shock and had to be excluded.)

The electroconvulsive treatment.

The electroconvulsive treatment (ECT) was started 8 hrs. after the last learning trial. Each animal in the experimental group had one shock a day for 6 days. The convulsion was induced with a 35 mA alternating current from the secondary of a transformer applied for 1 sec. between the rat's ears. A series resistance served to minimize variations in current strength due to changes in skin resistance. Saline-soaked clip electrodes were used. The current induced a convulsive syndrome of the same nature as previously described (e.g. Braun *et al.*, 1949). In order to insure similar conditions for control animals they were subjected to a dummy procedure which was the same except for the shock.

Retention trials.

24 hours after the last ECT the rats were returned to the maze. They were now given a free choice between the two habits previously learned. Trials were continued until each rat had made 20 runs.

III

Results

The mean number of trials needed to reach the criterion of learning habit I was $2·2 \pm 0·52$. The training was continued until each rat had made 120 runs after reaching the criterion. Thus a high degree of overlearning had taken place. The number of trials needed for learning habit II was $7·38 \pm 1·19$. The second habit was trained until each rat had made 30 runs after reaching criterion. The overlearning of habit II was thus considerably less than of habit I.

The difference in habit choice of experimental and control animals after ECT and rest period is described below. Habit preference, however, was not the only way in which the behaviour of the two groups differed. During the course of ECT the experimental animals started to display certain changes in behaviour. They became tense and irritable and difficult to handle. No attempt was made to analyse these symptoms which have been noted in connection with ECT by previous investigators (e.g. Hunter & Brady, 1951; Porter *et al.*, 1948).

Habit choice after ECT or rest period.

The maze behaviour of the control group after the rest period was extremely constant. 10 rats consistently chose habit II in all 20 trials, i.e. the preference was the same as immediately before the rest period. One rat chose habit II in the first 5 trials and then changed over to habit I. Contrary to the control group the experimental animals showed a pronounced change and variability in maze behaviour during the 20 post shock trials.

The choice of habits in experimental and control groups will now be compared using chi-square with Yates' correction as a test of independence. Table I shows the results in the first test trial. In the experimental group only 4 rats maintain the

recent habit while 9 reverse to the old habit. The value of $X^2 = 9{\cdot}41$ (df = 1, $p < 0{\cdot}01$). Thus the difference in habit choice between the two groups is significant. When chi-square is calculated for the habit chosen on 4 out of 5 consecutive trials (Table III) the value obtained is 4·53 ($p < 0{\cdot}05$), thus there is still a significant difference between the two groups.

The control and experimental animals thus differ in their habit preferences, the controls consistently choosing the habit that was dominant immediately before the rest period, while some of the shocked animals reverse to the old habit. The question is whether the behaviour of the experimental animals is to be interpreted as habit reversal or whether choice after shock treatment occurs at random. To test the latter alternative, chi-square was calculated between experimentally obtained values and values to be expected at random choice (Tables II and IV). The first test trial yields a value of $X^2 = 1{\cdot}23$. In the case of the habit chosen 4 times out of 5, $X^2 = 0{\cdot}00$. Thus no statistically significant difference can be demonstrated. The choice between old and new habits occurs as if at random and no experimental evidence of habit reversal is offered.

TABLES I – IV. X^2-ANALYSIS OF THE HABIT CHOICE.

E: Experimental group
C: Control group

o: obtained value
e: expected value

I: habit I
II: habit II

HABIT CHOICE IN FIRST TEST TRIAL

TABLE I

	I	II
E	9	4
C	O	11

$X^2 = 9{\cdot}41$
$p < 0{\cdot}01$

TABLE II

	I	II
o	9	4
e	6·5	6·5

$X^2 = 1{\cdot}23$
$p > 0{\cdot}20$

HABIT CHOSEN ON 4 OUT OF 5 CONSECUTIVE TRIALS

TABLE III

	I	II
E	6	7
C	O	11

$X^2 = 4{\cdot}53$
$p < 0{\cdot}05$

TABLE IV

	I	II
o	6	7
e	6·5	6·5

$X^2 = 0{\cdot}00$
$p > 0{\cdot}20$

IV

DISCUSSION

In interpreting the effect of ECT on habit preference there are two distinct questions to be settled, (*a*) is there a significant difference between the shocked animals and the controls and (*b*) if so, what change has occurred in the habit pattern of each group?

The results of all workers have shown (*a*) to be significant, but insufficient attention has been given to the analysis of (*b*). Previous investigators actually did no further analysis, merely concluding that the change of the shocked animals must be in the nature of habit reversal. When, however, the chi-square test is applied to their data in the same way as above to test deviation from random choice, no statistically significant difference is found. (Chi-square for Duncan's results yields a value of 2·45 ($p > 0{\cdot}10$). For Braun's and Patton's results, the corresponding value is 2·77

($p \sim 0{\cdot}10$). The control animals persisted in the habits most recently learnt, but shocked animals differed from them, not in reversal of habit but in loss of habit so that habit choice in the test trials became insignificantly different from random choice.

The fact that habit choice in all these instances is not distinguishable from random choice is not proof that habit reversal does not occur, or could not occur, under different experimental conditions. But the arguments needed to maintain such views now become more far fetched, and the more special the conditions necessary to demonstrate habit reversal the less useful it is likely to be in explaining clinical observations.

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THE RELATIONSHIPS BETWEEN P.G.R. SCORES AND TEMPERATURE AND HUMIDITY

BY

P. H. VENABLES

From the Medical Research Council, Unit for Research in Occupational Adaptation, Maudsley Hospital, London

Following an incidental finding that psychogalvanometer response measures were related to temperature and humidity readings taken at the same time, the nature of the relationship was examined in greater detail.

A sizeable relationship between temperature and conductance response measures is found only in a neurotic group and only above an effective temperature of 66° F. This relationship appears to be a function of the position of the response within the total experimental session, and may be dependent upon the relative excitation, or recovery from excitation, of the subject. Humidity in general, affects only the basal conductance level and, does so in both normal and neurotic groups. The relationship between temperature and humidity is probably a complicated one and cannot be corrected for by simple regression except if the majority of scores be within a particular range of humidity readings.

I

Introduction

Results of work using the psychogalvanometer as an instrument to investigate differential responses among normal and neurotic subjects were found to be subject to variation due to the influence of temperature and humidity. The work described here is the product of a detailed examination of relationships between P.G.R. measures and readings of temperature and humidity observed simultaneously. It is realised at the outset that the results reported here cannot be more than indications of interesting relationships which may be further investigated at a later date by experimentation where temperature and humidity can be subjected to control.

Not a great deal of work has been undertaken to determine the effects of climatic variables on the psychogalvanic reflex. What results have been achieved tend to be inconclusive and contradictory. This may be due to the variety of techniques employed in the measurement of the P.G.R. and because variables are more often than not considered as factors to be controlled or eliminated than variables to be examined in their own right.

Assessment of the findings reported in the literature to date is difficult in view of the variety of techniques used and the differences in ranges of climatic variables. Nevertheless an attempt will be made to find some pattern among the results reported, for this may help to clarify the present findings.

Kuno (1934) noted that his researches on human perspiration strongly support the view that the psychogalvanic reflex is due to activity of the sweat glands and states "A great number of observations have been made on the perspiration of the palm at high room temperature, and none has failed to confirm the view that it does not increase at all when a profuse sweating is produced on the general body surface." On the other hand he produced evidence to show that mental work does produce palmar sweating at ordinary room temperature. At higher temperatures (86° F.) mental work is said to have no effect on palmar perspiration but it may still increase sweating on the general body surface. Finally when room temperature is considerably raised (115° F.) mental work exerts a pronounced inhibitory effect on the sweating of the general body surface, while palmar perspiration remains uninfluenced or

shows a slight tendency to decrease. Conklin (1951) finds that skin conductance changes significantly for palms, wrist and forehead, with changes in effective temperature. At higher temperatures the conductance increases, at lower temperatures it decreases. Unfortunately Conklin used a rather restricted range of high effective temperatures (75·4, 80·5, 85·1° F.), and his conclusion, "That changes in effective temperatures are reflected in the level of skin conductance, regardless of area palmar or non-palmar," is of limited value.

Duffy and Lacey (1946) have published an incidental finding that basal resistance is not related to temperature. Testing was carried out at a temperature around that of the maximum temperature used by Conklin. This finding is supported by Cattell (1928), Smith (1937) and Freeman and Giffin (1939). However, the latter two studies are based on work on only six and one subjects respectively, and the last reports only the result of graphical examination of the data without statistical check. Wenger (1948) gives a considerable amount of data on the relationship between environmental variables and three P.G.R. measures. Working at about a mean temperature of 80° F. (exact means or standard deviations are not given), he found a correlation of +·06 between palmar conductance and temperature with 151 normal subjects. Unfortunately we do not know how far this small correlation is a function of a restricted range of temperature, or whether the high mean temperature leads to an effect similar to that reported by Kuno. A measure of Log Change in Conductance taken while the subject was under considerable muscular strain showed a correlation of −·29 with the temperature of the testing room. On a second occasion this dropped to −·086. Turning to the effects of humidity we find correlations of −·14 and −·089 between humidity and Palmar Conductance, and +·06 between humidity and Log Conductance change. No data for the mean or range of humidity measures are given. These findings of lack of relation between humidity and skin resistance are supported by the work of Smith (1937) and Freeman and Giffin (1939) cited above.

The findings of these workers may, it is thought, be taken to provide several general indications. If at all there may be a positive correlation between palmar-conductance level and temperature. When the subject is under some degree of excitation, either by performing mental arithmetic, or some muscular task, an increased temperature may be shown to have a depressing effect on the production of palmar sweat which would normally occur at ordinary room temperature. We may therefore expect a negative correlation between temperature and a measure of change of palmar conductance during a task. It should be noted, however, that the work cited only indicates expectation of these correlations in temperature above 75° F. and that the findings are from work carried out on purportedly normal groups. Wenger's work indicates some very low negative correlations between humidity and palmar conductance, and a further near zero correlation between humidity and low conductance change.

The work to be described was carried out on two groups of subjects, one a normal industrial sample, and the other a sample of neurotic patients in a psychiatric hospital. It was hoped to find whether the tentative expectations indicated from the work reviewed could be supported at temperature ranges more usually found in this country, and whether the findings in a normal group would be paralleled in a neurotic group.

II
Methods

Apparatus

The apparatus involved a valve-voltmeter type circuit operating a recording milliameter which gave continuous readings of the potential difference produced across a subject by the flow of a virtually constant current of 10 micro-amps. The readings of the

milliameter were directly calibrated in conductance units. The electrodes were 1 in. diameter plated discs set in sponge rubber 1¼ in. diameter placed on the palmar surface of the hand at the base of the thumb and on the volar surface of the same forearm just above the wrist. The electrodes were dry, i.e. no electrode paste or dressing was employed.

Details of Measurement

Percentage conductance change was adopted as a suitable measure for P.G.R. response as it was found to be independent of Basal conductance measures and gave scores which were normally distributed. Other recommended measures, c.f. Lacey and Siegel (1949), such as conductance change and log conductance change were not found to satisfy the criterion of independence.

Five P.G.R. measures were used, these were: Basal Conductance (BC), the highest level of conductance reached in the last minute of a 4-minute rest period, and four Percentage Conductance Change scores (PCC1 – 4) measuring change during a practice period and two testing periods on a psychomotor task, and during a subsequent two minute rest period.

In order to clarify future descriptions of the results obtained it is necessary to give a brief description of a psychomotor task referred to above. The task involved response requiring the movement of a pointer, under stimulus conditions which increased and decreased in complexity. Thus while PCC1 measured percentage conductance change during a practice period on the task, PCC2 measured change while the task stimulus conditions were increasing, and PCC3 while they were decreasing in complexity.

Measures of wet- and dry-bulb temperatures were taken at the time of connection of the electrodes, a whirling hygrometer being used for this purpose. These measures were converted into measures of Effective Temperature, and Relative Humidity using the tables and charts given by Bedford (1946). For this purpose the air velocity was assumed to be zero, as the work was carried out in a room with doors and windows closed.

Subjects

The normal industrial sample consisted of 210 male subjects with ages ranging from 18 to 46 (mean 28·1 ± 7·5) years. The patient sample comprised 52 psychoneurotic in-patients in a psychiatric hospital; their ages ranged from 19 to 48 years (mean 32·9 ± 8·5) years.

III

Results

The mean effective temperatures were 64·9° ± 2·38° F., and 66·36° ± 2·85° F. for the period during which the testing was carried out with the normal and neurotic groups respectively. The corresponding figures for relative humidity were 57·9 ± 5·74 per cent. and 60·2 ± 6·94 per cent. Temperature and humidity were normally distributed within the normal group, but the distributions for the neurotic-group test period showed some deviation from normality. In view of this, and the relatively small numbers involved in the second case, non-parametric statistics were used.

It was considered that effective temperature and relative humidity could be treated as independent variables in view of their insignificant correlation, —·133 in the normal and —·03 in the neurotic samples.

Ten scatter diagrams were drawn for each group to show the relationships between Effective Temperature, Relative Humidity and the five P.G.R. scores. Array means for temperature and humidity were determined, and the resulting plotted regression lines were examined for evidence of curvilinearity. In the case of the normal group, the regression lines showed insufficient evidence of curvilinearity to justify expenditure of work in testing for it. Product-moment correlation showed some relationships of a small nature which were in some cases significant with the large size of sample, but in no cases did temperature account for more than 3 per cent. of the variance of the P.G.R. scores. These coefficients are shown in Table I.

TABLE I

CORRELATION OF P.G.R. BASAL LEVEL AND RESPONSE SCORES WITH EFFECTIVE TEMPERATURE

Variable		Normal group n = 210		Neurotic group					
				n = 52 Total		n = 29 Above 66° F.		n = 23 Below 66° F.	
		r	P	tau	P	tau	P	tau	P
Basal conductance	BC	+·162	<·05	+·11	·61	−·15	·68	−·10	>·9
Percentage conductance change									
Motor task practice period	PCC1	−·141	<·05	−·25	·14	−·48	·06	+·08	>·9
Motor task, easy-difficult	PCC2	+·127	>·05	+·01	>·9	−·33	·16	·01	>·9
Motor task, difficult-easy	PCC3	+·115	>·05	+·17	·34	+·05	>·9	·03	>·9
Second rest period	PCC4	−·107	>·05	+·40	·01	+·51	·02	·06	>·9

With the neurotic sample, evidence of curvilinearity was shown on some of the scores. In view of the small numbers in the sample and the distribution characteristics, it was decided to seek statistical evidence of curvilinearity by using Kendall's rank correlation method (tau) as applied to a fourfold table by Whitfield (1947). The P.G.R. score was dichotomized at the mean, and the temperature at 66° F., which in addition to being the mean temperature for the group appeared to be the point of inflection of the regression lines. A correlation coefficient was calculated for this fourfold table.

Following this, two similar coefficients were calculated for the temperature ranges above and below the mean point 66° F.; the upper range being dichotomized at 67/68° F. and the lower at 64/63° F. The resulting correlations are shown in Table I. There it will be seen that below 66° F. there is in no case any correlation between temperature and P.G.R. scores. However, above that point, correlations range from −·48 to +·51 between percentage conductance change score and temperature. An interesting feature which may be of relevance is the change in sign of correlation from negative to positive in succeeding periods. This should be considered in relation to the probable increase in the subjects' excitation in periods 1 and 2, and the decrease in periods 3 and 4.

Confirmation of the correlations between PCC1 and temperature is suggested by data kindly supplied by Mrs. Sybil Eysenck, with appropriate conversion to allow for difference in units used. The correlation in a neurotic group between temperature and P.G.R. response in the first minute after the end of an initial rest period is −·42 ($P < ·05$) above 68° F. dry bulb temperature, and −·25 (NS) below 68° F. A dry bulb temperature of 68° F. is of similar order to an effective temperature of 66° F. In a normal group there was no evidence of curvilinearity.

Examination of the scatter diagrams showing relationships between Relative Humidity and percentage conductance change scores showed no evidence for curvilinearity on either the normal or neurotic groups. Table II shows the product moment correlation on the normal group and the rank correlation on the neurotic group between percentage conductance change scores and relative humidity over the total range of the latter variable.

TABLE II

Correlation of P.G.R. Basal Level and Response Scores with Relative Humidity

Variable	Normal group n = 210		Neurotic group n = 52	
	r	P	tau	P
BC	$-\cdot623$	$<\cdot001$	$-\cdot21$	$\cdot23$
PCC1	$+\cdot016$	$>\cdot05$	$-\cdot14$	$\cdot48$
PCC2	$-\cdot001$	$>\cdot05$	$-\cdot21$	$\cdot21$
PCC3	$+\cdot021$	$>\cdot05$	$-\cdot49$	$\cdot001$
PCC4	$+\cdot118$	$>\cdot05$	$+\cdot09$	$\cdot68$

No significant relationships were found with data from the normal group; and only in the case of PCC3 was a significant relationship found in the neurotic group. It is of interest to note that this was a conductance change score on which no relationship with temperature was shown; it may be that the effect of humidity is secondary to that of temperature and is evident in the absence of prior effect of temperature.

Examination of the regression of basal conductance on relative humidity suggests that there is curvilinearity of such a nature that there is a positive correlation between humidity and basal conductance above 66 per cent. RH; between 66 per cent. and 54 per cent. this correlation becomes negative; and below 54 per cent. the correlation is again positive. The negative correlation in the central range of humidity is quite clear; in the normal group tau $= -\cdot30$, $P = \cdot00009$; the neurotic group tau $= -\cdot26$, $P = \cdot051$. The apparent positive correlation above 66 per cent. may be due to chance and is based on too few figures to allow a test of significance; it is, however, of a similar nature in both groups. Below 54 per cent. there is a rank correlation of $+\cdot21$ ($P = \cdot31$) based on 20 cases from the normal group. The product-moment correlation of $-\cdot623$ between humidity and basal conductance in the normal group is no doubt due to the fact that the variance in humidity scores restricts the range to that mainly covered by the negative regression. A lower product-moment correlation of $-\cdot148$ on the neurotic group is possibly found because the rather non-normal distribution capitalizes on the positive correlation at the tail of the humidity distribution. The variance of humidity scores in the neurotic sample is $1\cdot48$ times as great as that in the normal group.

IV

Discussion

These findings present some problems which further investigation may clarify. At the moment only speculation is possible. It is suggested that the finding that there is a point at which neurotics are affected by temperature, whereas normals do not show such a point, is because the threshold for normals was not reached in the range of temperatures involved. The maximum effective temperature encountered was 73° F. The work of Conklin and Wenger reviewed in the introduction shows that in manifestly normal subjects temperature does affect P.G.R. response measures at some point above 75° F.

The change in sign of correlation with temperature at different stages throughout the session in which the P.G.R. measurements were taken poses a very interesting problem. One hypothesis which can be advanced is that the neurotic, in addition to being sensitive to the effects of temperature at a lower point than normal, is further

handicapped by a deficiency of excitatory potential and exhibits a mechanism analogous to Skinner's (1938) "reflex reserve." The negative correlations between increase in conductance and temperature in the practice and easy-difficult periods of the motor task would then appear because the increase in motor excitation required for those conditions made itself felt in a deficiency of autonomic excitation. In the difficult-easy period of the motor task and the subsequent rest period, when the P.G.R. response-temperature correlation coefficient changes to positive, the organism is less concerned with motor excitation and is able to divert its potential into autonomic channels. Wenger's work shows a similar negative correlation (—·29) between log conductance change during a period of muscular tension in a normal group, at a higher temperature; but his work did not provide a period which might have given confirmatory evidence for a positive correlation in a rest period.

A further point of interest is the significant correlation of PCC3 with humidity in the neurotic group, this being the variable on which no effect of temperature is shown. A possible explanation is that humidity appears as an effect secondary to temperature where response scores on a neurotic group are concerned. The effect of humidity on basal conductance is not clear, and, although in general the correlation between these variables seems to be negative, the suggestion that there is a positive correlation above 66–68 per cent. relative humidity seems to be more susceptible of a common sense explanation. Blank and Finesinger (1946) show that an aqueous film such as sweat, acts to increase the effective size of an electrode, thus if evaporation is prevented by the high humidity of the atmosphere it would be expected that a higher conductance would result. It is suggested that the non-linear regression found is a product of the interaction of opposing forces. The negative correlation may be due to some physiological mechanism, the positive to a simpler physical one. A physiological mechanism which gives a possible explanation of the negative correlation reported above is that described by Dugge (1928) who states that damp weather produces high palmar resistance due to thoracic pressure on the vagus nerve.

These findings resulting from observational rather than experimental climatic data cannot pretend to be conclusive. However, they appear to be of sufficient interest to warrant further investigation.

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THE ROLE OF SPATIAL STIMULI IN THE PERCEPTION OF SHAPE.

PART I.

BY

J. LANGDON

From the Institute of Experimental Psychology, University of Oxford

Arising from earlier studies of shape constancy (Langdon, 1951; 1953) an attempt is made to examine the rôle of spatial-surround cues in shape perception, as distinct from cues provided by the object. A series of shapes is compared in total darkness, firstly with shapes stationary and secondly with shapes rotating continuously. The findings are compared with those obtained in normal illuminated surrounds. The results indicate that surround cues play an important part in perception of orientation, and hence perception of shape. An explanation is suggested involving the use of surround cues as "anchorage" or "reference" frames. This is investigated further in Part II of this paper by an attempt to modify the character of the surround cues in a controlled way.

Introduction

A recent paper by the present writer (Langdon, 1953) attempted to examine the theory of invariance originally proposed by Koffka (1935) as applied to the perception of shape and orientation. In the course of this investigation comparisons were made between procedures using stationary objects and others involving continuously rotating forms, and it was shown that differences exist between these two types of situation.

It was considered nevertheless, that none of these differences prevent the second type of situation being regarded as equivalent to the first. On the contrary, the essential aspects of the constancy situation were preserved and enhanced. Subjective psychological factors were stabilised and conflicting supplementary cues emanating from the surfaces of forms could be removed whilst the level of constancy and accuracy of estimation of shape was raised.

At the same time, inspection of actual scores reveals discrepancies which, whilst in no way vitiating the conclusions of the study, do suggest a further line of enquiry. In Experiment Series 1 (p. 94) the control situation was inspection of the forms exhibited as silhouettes in a darkened room. For this condition all subjects made an estimate corresponding to stimulus equality (retinal image), and this was interpreted after the manner of Thouless (1931) as due to the absence of textural cues giving information on the slant of the surface seen in projection. Later in the same study stationary wire-outline forms were viewed under normal illumination by subjects, resulting in the following estimates:—

TABLE I

Constancy Values for Wire Outlines and Solid Shapes Matched in step-by-step setting for 10 Subjects.

	80°	70°	60°	50°	40°	30°	20°	10°
Wire Outline ..	0·25	0·22	0·17	0·18	0·11	0·08	0·06	/ /
σ S.E. ..	2·4	4·05	5·5	6·76	4·24	4·12	4·06	
Solid Shape ..	0·29	0·25	0·19	0·19	0·1	0·07	0·05	/ /
σ S.E. ..	2·12	2·23	3·08	3·16	4·0	4·06	3·87	

N = 320

In previous papers (Langdon, 1951; 1953) it was shown that in the absence of textural cues, perception of tilted shapes approached retinal-stimulus value when the shapes were stationary. Since rotation of the shape tended to restore perceptual constancy it was tentatively suggested that the gradient of retinal stimulation thus produced was the sole determinant in perception of real shape. The data of Table I, however, make it clear that such an hypothesis is over-simple, for they show that constancy is still present for wire outlines even when these are stationary.

Neither the silhouettes nor the wire outlines afford direct cues to judgment of their orientation (and hence, their shape), and in this respect the two situations are similar. Nevertheless, total field conditions are completely different, for the silhouettes were viewed with much reduced room lighting, thus reducing spatial cues, whereas the wire forms are seen under full illumination and the frontal-parallel plane is readily located through strong perspective indications. Thus while object cues might be slight, there remained strong anchorage from points of reference within the general visual field which were absent in the case of silhouettes viewed in dim light. This suggests the need to study the possible rôle of the spatial surround.

In discussing phenomenal constancy, Koffka (1935; 1932) laid stress on the importance of the visual framework, citing the work of Wertheimer (1912), who demonstrated the righting, with passage of time, toward true vertical, of the image of a room seen aslant through a tilted mirror. The controversy between Gibson & Mowrer (1938) and Asch & Witkin (1948) gave impetus to detailed studies of visual reference frames, whilst another aspect of the problem was demonstrated by Rubin's (1927) study of the perception of motion in dark space. Rubin showed that a stationary point surrounded by a moving framework was experienced as in motion, since the framework was accepted as the visual reference for the smaller area of stimulation provided by the luminous point. At the same time Guilford & Dallenbach (1928) investigated the autokinetic effect and in turn showed that without a visual frame a point source could not be localized, whilst Duncker (1929) developed a number of ingenious demonstrations of the dominant rôle of the spatial surround.

Acceptance of the general concept of "frame of reference" has become universal, independent of the psychological orientation of different groups of theorists. Thus Gibson (1950; 1951) has given a general formulation in developed Gestalt terms, whilst Ames and his associates makes it the basis for "assumptionist" views (*cf.* Ittelson, 1951a; 1951b) in sharp opposition to Gibson. In turn, Piaget (1948; with Lambercier, 1951) has attempted a fusion of both approaches, suggesting the progressive "axiation" of visual space through the internalization of movements of objects. The creation of such a pre-concept or "schema" makes possible the integration of momentary stimuli with the distinctions of up and down, straight-ahead and side to side, within which the visual object is orientated. More specifically, Asch & Witkin have shown that accuracy and reliability of spatial orientation of objects depend upon the presence of a visual frame as "anchorage," independent of sensory information from other modes.

However, it may be noted that there exist important differences between the stimuli involved in the present experiments and those of the general literature of spatial orientation. In the first place there are no conflicts between the sensory modes as in the case of Wertheimer's studies, or those of Asch & Witkin where objects were perceived with body tilted, and in the second place the stimuli are not simple lines or points of light. Thus in the case of the stationary outline there arises a retinal projection, and in the case of rotation a deformation of the retinal image corresponding to a three-dimensional form. These distinctions will become clearer as the discussion proceeds.

But it would seem, from what has already been said, that a repetition of earlier experiments under more rigidly controlled conditions and in the absence of all spatial cues might help to establish conclusively (*a*), the rôle of rotary movement in perception of the true shape of the form, (*b*) conversely, to demonstrate the importance of general space cues in normal perception, and (*c*) provide additional corroboratory data as regards the theory of invariance (i.e., changes of constancy through the arc of orientation).

Nevertheless, such evidence could not, by itself, be regarded as conclusive in demonstrating that rotary movement provides the means of appreciating the true shape of a form. For such evidence would only indicate that in the *absence* of general spatial cues these object variables were effective. It would give no indication of the rôle of the latter in the presence of normal space cues, of whether the "information" supplied by the object and by its setting is integrated, and whether the congruence of these two groups of stimuli is in fact necessary to normal veridical perception. For to say that perception of shape is in any way dependent upon general space cues raises at once the whole problem of the conventional definition of shape.

The ordinarily accepted definition of shape is not concerned with its spatial setting, but the full significance of this concept cannot be realized without resort to characteristics of the surrounding visual and spatial field. Insofar as the space realized by perception is one in which physical events take place (and thereby define the space), a definition of shape requires to be qualified by reference to the space in which it has its being, in which it is set. But if physical (and visual) space is realized solely by events (or the activity of objects) it cannot be explicitly defined since there is nothing "outside" this activity with which to contrast the internal description of space.

To carry out an experimental definition of shape in terms of space it is therefore quite useless merely to eliminate stimuli giving rise to visual space, since if the hypothesis already put forward is correct, self movement of the object will tend to give rise to the space of physical movements (euclidean) whilst a stationary object in a dark field results in a retinal projection (perspective). Such a study requires the presence of a space which, whilst tridimensional, is nonetheless different from normal perceptual space. In such an artificial space the apparent properties of the visual surround may possibly conflict with the visual appearance of the rotating object, insofar as the rotation tends to produce perceptual constancy—in other words, constancy of size and shape appropriate to normal perceptual space.

Results obtained under such conditions might indicate whether the perceptual effects due to the motional cues are autochthonous to the object and independent of the surround cues, or whether there is some necessary linkage between the two groups of stimuli. It is therefore intended to describe two groups of experiments designed to investigate these various hypotheses. In the present paper (Part I), a direct comparison will be made between stationary and rotating wire outlines in dark space, while in Part II an attempt will be made to relate such comparisons carried out within an equivalent space.

EXPERIMENTS, SERIES I

Aim of Experiment

The main aim of this set of experiments is to study the influence of perceptual cues emanating from the object as distinct from those of the surrounding field, and to carry out at the same time a check on the fate of shape constancy through the arc of orientation as reported previously, but under these more rigorous conditions. With this last aim in view it was proposed to employ new points of comparison within the arc, intermediate between those used in the earlier experiments.

Method

The experiment was performed in a darkened room 15 by 12 feet, the walls of which were covered with black paper. A larger room would have been more suitable, but the combination of large size with total darkness was beyond laboratory resources. The stimuli were wire forms in the shape of a circle and ellipse, the rotating circle having a diameter of 17 cm. and being separated from the comparison shape by an angle of 10° from the viewpoint of the subject, who was placed some 2·5 m. from these shapes. The subject was seated upon a stool having no side arms or supports, with his head resting against the near wall. The shapes were viewed at eye level. To render the stimuli visible in dark space these were coated with a fluorescent medium and exposed to a small amount of U.V. radiation. The brightness of the shapes was controlled by shutters in the housing of the U.V. lamp and luminosity was maintained just slightly above visual threshold. As the object was seen by its own light the laws of reflection do not apply to this case, and thus rotation of the circle yields no gradient of luminosity, change of outline being the only available cue to shape. Frameworks were also placed round the object to act as baffles for any stray light or U.V. radiation so that nothing but the narrow outline was visible to the subject. The method of rotating the wire circle has already been described (Langdon, 1951), the only change from the previous technique being that a clutch mechanism was inserted in the drive. When this was disengaged the subject could himself set the circle at any desired position by means of a small "Magslip" remote control, or the experimenter could similarly adjust it under the subject's instructions. Thus the subject was able either to make an estimate of similarity of shape with the form rotating at a known speed, or set it himself and compare two stationary shapes.

Description of Experiment

Five wire outline shapes corresponding to projections of a circle 17 cm. in diameter at angles of 35°, 40°, 45°, 50° and 55°, were constructed and coated with fluorescent paint. As the variable object the circle was rotated at five different sweep rates of 90, 75, 60, 45 and 30 seconds for one excursion of 90°. Monocular vision was used throughout, one eye being covered with a shade. Twelve subjects were used, five of whom had taken part in similar experiments using normal surrounds. Of these twelve, six were men and six women. Two were Psychology students, four were University Arts students, two were housewives and four were professional workers. Three used spectacles to correct their vision (none were astigmats).

The subject was given an eyeshade and guided to his seat, seated and his position checked. Asked what he could see he usually answered "nothing at all." The shutters of the U.V. lamp were then opened until the subject reported seeing two shapes. They were then closed slightly and the push button was placed in the subject's hand. He was then asked to attempt to match the pair of shapes by pressing the button whenever they appeared most similar, and the apparatus was set in motion. For each comparison figure, eight trials at each velocity were given, four in each direction. After about 10 minutes the subject was led from the room into full light whilst the comparison shape was changed for another. Shapes and speeds were given in random order and exploration of the complete series spread over a number of sessions. During one session the shapes were matched using step-by-step setting or by the subject making settings for himself. The subject's remarks were recorded on tape for future study.

Results of Experiments

The results of this series are given below (Tables II–VI) in tabulated form. First, the subject's general remarks and comments will be dealt with. The rotation of a circular outline under conditions such as these appears to have a peculiar effect. Absence of visible changes in illumination, texture or any aspect of the outline other than progressive deformation appears to suggest that the change is physical and not the result of spatial orientation. The hue or brightness of the stimulus objects appears to play no part in this, for experiments in which hue and brightness were changed yielded similar results.

After 10 minutes of darkness the subject could give some vague notion of the room he was in though no subject could identify any part of it, or anything within it.

TABLE II

SHAPES MATCHED AT 4 SPEEDS, 30, 45, 60 AND 90 SECONDS FOR 90° SWEEP.

Subject	55° *In*	55° *Out*	50° *In*	50° *Out*	45° *In*	45° *Out*	40° *In*	40° *Out*	35° *In*	35° *Out*
1 ..	0·37	0·46	0·28	0·28	0·22	0·22	0·22	0·30	0·15	0·18
	0·286	0·288	0·225	0·225	0·088	0·133	0·16	0·24	0·090	0·126
	0·172	0·256	0·175	0·200	0·044	0·088	0·16	0·20	0·036	0·128
	0·086	0·114	0·075	0·075	0·044	0·088	0·08	0·140	0·018	0·072
2 ..	0·31	0·43	0·25	0·28	0·22	0·27	0·24	0·34	0·22	0·31
	0·230	0·342	0·150	0·250	0·133	0·199	0·18	0·32	0·110	0·274
	0·143	0·172	0·125	0·200	0·066	0·155	0·12	0·26	0·072	0·238
	0·057	0·114	0·100	0·175	0·066	0·111	0·08	0·18	0·054	0·202
3 ..	0·11	0·17	0·18	0·15	0·07	0·09	0·10	0·08	0·09	0·11
	0·143	0·114	0·75	0·125	0·022	0·066	0·12	0·12	0·072	0·110
	0·057	0·114	0·75	0·050	0·044	0·044	0·06	0·14	0·018	0·056
	0·028	0·086	0·025	0	0	0	0	0·04	0	0
4 ..	0·17	0·26	0·23	0·25	0·2	0·2	0·18	0·22	0·15	0·18
	0·114	0·143	0·100	0·200	0·156	0·177	0·12	0·10	0·054	0·090
	0·057	0·057	0·025	0·050	0·044	0·088	0·06	0·10	0·036	0·072
	0	0	0	0	0	0	0	0	0	0·018
5 ..	0·26	0·34	0·23	0·25	0·16	0·2	0·18	0·20	0·07	0·13
	0·114	0·200	0·100	0·175	0·133	0·199	0·16	0·14	0·128	0.146
	0·057	0.114	0·050	0·125	0·133	0·177	0·02	0·08	0·054	0·110
	0	0·028	0	0·025	0·022	0	0	0·02	0·036	0·036
6 ..	0·03	0·17	0·03	0·18	0·11	0·16	0·06	0·10	0·07	0·09
	0	0·086	0·050	0·125	0·066	0·111	0·06	0·06	0.054	0·090
	0	0·057	0·025	0·100	0·044	0·088	0·02	0·04	0·036	0·072
	0	0·028	0	0·025	0	0·044	0·04	0·06	0·036	0·036
7 ..	0·46	0·37	0·40	0·33	0·38	0·36	0·34	0·32	0·31	0·35
	0·314	0·430	0·300	0·325	0·266	0·311	0·28	0·34	0·274	0·292
	0·286	0·400	0·225	0·275	0·177	0·288	0·12	0·80	0·164	0·312
	0·200	0·371	0·125	0·275	0·133	0·177	0·10	0·18	0·146	0·384
8 ..	0·20	0·20	0·18	0·23	0·11	0·14	0·10	0·16	0·11	0·18
	0·114	0·172	0·100	0·175	0·044	0·088	0·02	0·06	0·036	0·072
	0	0·057	0·075	0·100	0·022	0·088	0	0·08	0	0·054
	0	0·057	0	0·050	0	0	0	0·08	0	0
9 ..	0·37	0·43	0·25	0·28	0·18	0·29	0·18	0·26	0·09	0·27
	0·314	0·256	0·125	0·225	0·066	0·222	0·08	0·22	0·072	0·164
	0·086	0·057	0·75	0·100	0·044	0·088	0·04	0·10	0·036	0·012
	0	0·028	0	0·025	0	0·44	0	0·02	0	0·018
10 ..	0·23	0·23	0·15	0·23	0·11	0·13	0·10	0·12	0·11	0·13
	0·114	0·172	0·150	0·225	0·088	0·133	0·08	0·12	0·072	0·110
	0·028	0·114	0·125	0·175	0	0·088	0·06	0·06	0	0·090
	0	0·086	0	0·175	0	0	0·02	0·14	0	0·054
11 ..	0·23	0·26	0·28	0·15	0·11	0·09	0·12	0·14	0·05	0·07
	0·057	0·342	0·275	0·150	0·044	0·066	0·06	0·10	0·090	0·018
	0	0·172	0·225	0·125	0·066	0·022	0·08	0·14	0·018	0
	0·018	0·051	0	0·075	0	0·066	0	0·04	0	0·018
12 ..	043	0·45	0·37	0·37	0·29	0·36	0·26	0.30	0·31	0·29
	0·371	0·342	0·375	0·450	0·222	0·266	0·20	0·22	0·182	0·256
	0·400	0·342	0·275	0·400	0·244	0·355	0·16	0·16	0·146	0·164
	0·286	0·342	0·300	0·375	0·311	0·288	0·18	0·22	0·090	0·164
Means	0·27	0·31	0·24	0·25	0·18	0·21	0·18	0·21	0·15	0·19
	0·172	0·243	0·175	0·225	0·066	0·166	0·12	0·17	0·110	0·146
	0·114	0·143	0·125	0·150	0·066	0·133	0·08	0·14	0·54	0·110
	0·057	0·114	0·050	0·100	0·044	0·066	0·04	0·10	0·036	0·090
Combined Means for Speed 1 (30 seconds sweep)	0·290		0·245		0·195		0·195		0·17	

Subjects 12. N = 384.

TABLE III

MEAN CONSTANCIES FOR FIVE SHAPES AT FIVE VELOCITIES, SUMMARIZED.

Velocity	55° In	55° Out	50° In	50° Out	45° In	45° Out	40° In	40° Out	35° In	35° Out	Velocity Sweep Rate
1 ..	0·06	0·11	0·05	0·10	0·04	0·07	0·04	0·10	0·04	0·09	90 seconds
2 ..	0·05	0·09	0·06	0·11	0·05	0·09	0·08	0·11	0·07	0·10	75 ,,
3 ..	0·11	0·14	0·13	0·15	0·07	0·13	0·08	0·14	0·06	0·11	60 ,,
4 ..	0·17	0·24	0·18	0·23	0·07	0·17	0·12	0·17	0·11	0·15	45 ,,
5 ..	0·27	0·31	0·24	0·25	0·18	0·21	0·18	0·21	0·15	0·19	30 ,,

SUBJECTS 12. N (FOR ALL CONDITIONS EXCEPTING 2) = 1920

TABLE IV

MEAN SPACE ERRORS FOR EACH CONDITION. VALUES GIVEN IN DEGREES OF ARC

Velocity	55°	50°	45°	40°	35°	Velocity Sweep Rate
1 ..	2	2	1	3	3	90 seconds
2 ..	1	1	2	2	2	75 ,,
3 ..	1	1	3	3	3	60 ,,
4 ..	3	2	3	3	2	45 ,,
5 ..	2	2	1	2	2	30 ,,

TABLE V

MEAN SPACE ERRORS FOR EACH SUBJECT IN DEGREES OF ARC. FROM DATA OF TABLE II

Subject	55°	50°	45°	40°	35°
1 ..	4	1	2	5	3
2 ..	2	1	1	1	1
3 ..	3	1	0	2	2
4 ..	4	1	2	1	3
5 ..	5	6	2	2	1
6 ..	3	3	1	1	2
7 ..	0	2	1	4	4
8 ..	2	1	5	4	10
9 ..	0	3	0	1	1
10 ..	1	5	1	1	1
11 ..	1	0	4	2	1
12 ..	3	0	1	4	2
Means ..	2° 35″	2° 20″	1° 40″	2° 0″	2° 20″

Few could guess with any accuracy or certitude the size or distance of the forms. With the shape in motion the usual experience (spontaneously remarked) was of an object being "squeezed in and out." Only later did the subject report "something turning round." Some subjects stated that they could perceive either phenomenon at will.

As with earlier experiments it was found that the dominant error for two directions of motion was space error (nearly always positive in sign), whilst sequential errors for a given direction were extremely small. Therefore, the space error has been calculated simply by taking the mean of all subjects for a given shape, and the dispersions of sequential errors for a given shape by summing the dispersions for each subject. It may be seen from Tables IV and V that errors do not change significantly with change of orientation.

A comparison of Table VI with Tables III and I shows removal of supplementary surround cues to have been complete, resulting in stationary forms being perceived with zero constancy, i.e. projectively. Thus presence of constancy in Table II can be definitely ascribed to the effect of rotation and rise of constancy with speed to increased perception of motion.

TABLE VI

STATIONARY WIRE OUTLINE SHAPES IN DARK SPACE. SET BY HAND. MATCH SCORES IN DEGREES OF ARC FOR 12 SUBJECTS. THOULESS VALUES FOR THE COMBINED MEANS

Subjects	55° In	55° Out	50° In	50° Out	45° In	45° Out	40° In	40° Out	35° In	35° Out
1	56	56	52	53	44	46	39	41	34	34
2	54	55	50	51	46	45	39	39	36	37
3	56	57	50	52	47	46	41	40	38	36
4	52	52	45	45	40	39	34	34	28	28
5	56	56	51	50	45	45	41	40	34	34
6	56	55	50	50	44	46	40	40	35	36
7	55	56	50	51	44	47	40	42	36	36
8	54	55	51	50	45	45	40	41	35	35
9	56	54	50	51	45	45	40	40	35	35
10	55	55	51	51	44	45	40	41	35	36
11	55	54	50	50	46	45	41	41	34	35
12	56	55	52	51	46	46	43	41	36	35
Mean	55	55	50	50	45	45	40	40	35	35
Mean excluding 4	56	55	51	51	45	45	41	41	36	36
Constancy	0·029		0·025		0		0·02		0·018	

Subjects 12. N = 960.

If Table II is compared with Table I it will be seen that the general contour is maintained over the arc of tilt. The values interpolated for angles of 55°, 45°, and 35° are in general agreement with the points suggested by the path of the contour drawn in the earlier study, tending to strengthen the assumption that a smooth curve can be drawn for the whole of the tilt arc. It will also be noted that the phasic irregularities are again present, taking the form of changes in sign around the middle regions of the arc.

In the absence of a clear hypothesis giving expected values for the points along the contour (as would result from a valid formulation of the shape-tilt relation), or the expectation of the group of subjects yielding a normal distribution for their pooled mean scores, can the observed changes in direction be taken as significant in any meaningful way?

This question arose in the previous study and was attemptedly answered by means of a crude test of significance (1953, p. 105). A rough answer is that changes in constancy significantly larger than the space error for any subject, similar in sign and occurring in the same region of the contour for all experiments must be considered as significant. Thus correlation of all contours would yield a high positive value.

At the same time it will be seen that not every subject shows the same pattern of reaction. Nevertheless, inspection of Table II shows that whilst only two subjects (Nos. 3 and 4) show results markedly different from the remainder between 55° and 60°, no subject fails to show a change in constancy at the midpoint of the arc.

Discussion

It may be granted that the experiment demonstrates conclusively that in the normal process of shape perception the orientation of the object is perceived by virtue of surround cues in relation to stimuli emanating from the object itself. It was because such surround cues were present that a degree of constancy was obtained using stationary wire outlines. For this experiment was conducted in a normally illuminated room.

Thus it would seem that general stimulation from the surrounding field gives a sense of general orientation, an "anchorage" to employ Sherif's term, for the points or planes of reference in terms of which the object is perceived. In the absence of direct and unambiguous cues from the object itself, it is estimated as having some orientation, the reference points for which are derived from the surround. The removal of visible surround cues cuts out the last possible source of information on which an estimate of tilt could be based, and the stimulus object is therefore seen purely phenomenally (i.e. as a retinal projection). As already suggested (1951) rotary motion has the effect of supplementing this remaining variable, regular deformation of the stimulus providing axes of orientation and enabling the outline to be envisaged as occupying the line of regard, thus acquiring the perceptual character of a real three-dimensional object having spatial orientation.

As regards the second and subsidiary aim of the experiment, the repetition considerable variations in constancy around the arc of tilt, together with the phasic irregularities at the mid-region of the contour, would appear to make the development of any simple invariant shape-tilt formula even less probable. Even if the results of the present series are treated similarly to the earlier ones [by means of the formula (P-S/R-S).Cos.S.], the result is not a straight line but a curve which requires treatment by a cubic function to reduce it to a straight line. Whether this complicated and dubious set of procedures comes within the ambit of what had been envisaged by Koffka as a simple reciprocal relation seems very much open to question. It would seem more probable that inability to state the perceptual relationships by means of a direct and simple equation is evidence that the interaction of numerous factors, some of them subjective and variable, is involved.

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THE ROLE OF SPATIAL STIMULI IN THE PERCEPTION OF SHAPE.

PART II.

BY

J. LANGDON

From the Institute of Experimental Psychology, University of Oxford.

Subsequent to the experiment described in Part I, the rôle of spatial-surround cues is further investigated by attempting to modify the character of the environment. Pairs of shapes are matched, both stationary and rotating, within an Ames-type equivalent space. These shapes are of two kinds. The first is calculated for normal perceptual space, and the second pair are comparable only in the equivalent space. Comparison of matchings indicates that the perceived shape is a product of interaction between cues emanating from the object and the surround, in terms of which the object cues are evaluated, though the concept of "anchorage" or "reference frame" is distinguished from suggestions advanced by some authors.

Aim of Experiment

The experiment described in Part I showed that surround cues normally play an important part in shape perception, and also that under certain conditions accurate estimates of real (as opposed to projective) shape can be made by the observer independent of such cues. But this experiment did not show how these two types of perceptual variable are related; that is to say, whether the rotating motional "object cues" are independent of "spatial surround cues" and contingently complementary to them, or whether the surround cues form the basic anchorage points for the tilt reference frame. It is already well known since the work of Asch and Witkin (1948) that surround cues are indispensable for correct location of stationary object configurations. In the present case, however, the central feature of self-movement in the object raises this problem in a new way.

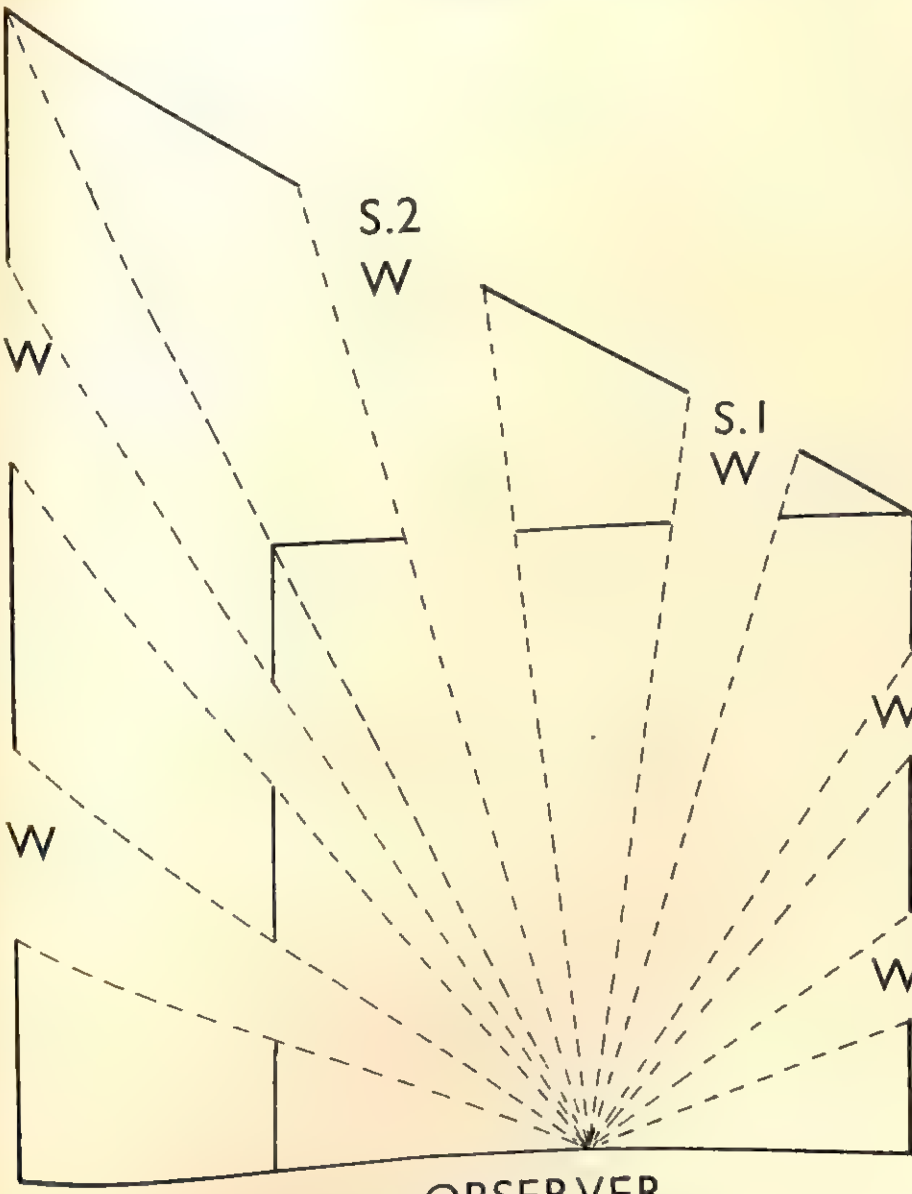
It should be clear at the outset that since both object and surround cues are necessarily complementary for euclidean and for normal perceptual spaces, the problem can only be investigated experimentally with the aid of a technique which might bring the two groups of variables into conflict.

This requirement can be met, as stated above, by the utilisation of a space which though 3-dimensional is nevertheless non-complementary. The term, complementary, is used here to indicate a space in which objects retain a constant shape and size irrespective of their position and orientation within the space. In non-complementary space it may be possible to produce a situation of *spatial conflict* since the rotatory two-dimensional forms will tend to generate a spatial axiation of euclidean character, whereas the surround cues suggest a space which though equivalent or complementary to euclidean space from some unique viewpoint is in fact anisotropic; that is to say, non-complementary as a whole. Thus apparent size and shape of objects will tend to vary according to their position and orientation within such a space. Hence the physical appearance, in terms of shape and size, of such objects would be incompatible with their spatial properties as suggested through their self-movement.

Ames (1951) and his associates have described a number of such constructions tending to induce anomalies of apparent shape and size. Similarly, Ittelson (1952) has reported demonstrations of conflictful situations such as movement of a playing card along a path tangential to the observer which occurs within a trapezoidal

"window frame." The trapezoid is perceived as a rectangular frame at an angle to frontal-parallel and the card is seen to meander in and out of the frame along a curved path. Ames (1951) describes the conflicts induced by the effect of a rotating

FIGURE 1



Plan projection of "equivalent" space showing window orifices and stimuli.

trapezoidal window frame, and has also shown (1946) that similar phenomena may be produced with the aid of aniseikonic lenses in both normal and abnormal environments.

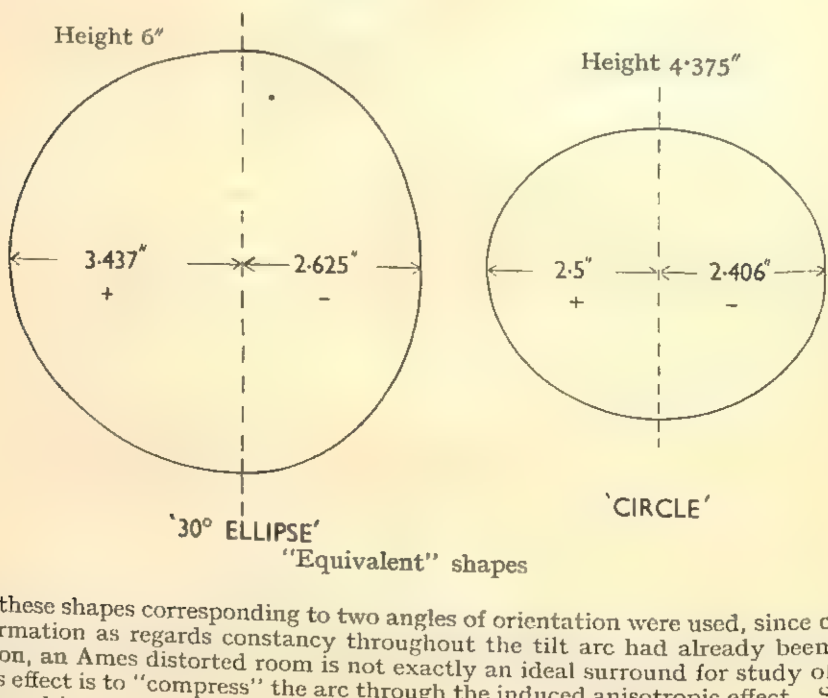
Hence it would seem quite feasible to define experimentally some properties of shape in terms of an explicit space by using a non-euclidean space as a basis for the subject's reference frame. The experiments will therefore consist of repeating earlier observations under such conditions as enable comparison to be made between normal visual space, dark space and abnormal visual space.

Method

It was decided to construct an Ames-type equivalent space with a rotation of the Frontal Parallel Plane of 30°. To do this in the ordinary way would have been expensive and laborious and it was therefore decided to dispense with actual walls and floor, thus eliminating the difficulties of carpentry, etc., and to construct the room from strings coated with fluorescent material. These strings, stretched on a frame to represent the edges of the walls, were irradiated with Ultra Violet and produced the luminous outline of a room having windows, ceiling, floorboards, wainscote, etc. The subject looked in the front, which was covered to obscure the rest of the construction. This also ensured that the room and objects could be seen only from the intended viewpoint. An adjustable wooden platform enabled subjects of different heights to see through the aperture without discomfort. (*See* Fig. 1.)

The stimulus objects used were of two kinds. The first corresponded to shapes which would appear, assuming an induced change in shape and size of the degree expected with such a rotation, as a circle and an ellipse when placed on the (apparent) frontal-parallel plane. The ellipse represented the declination of the circle to an angle of 30°. The second type was corrected for induced size changes so that the pair of shapes would appear equal in size, but were not corrected for induced shape changes, being calculated as a circle and its declination to 40° in normal space. The shapes were constructed as wire outlines 1 mm. thick and sprayed with fluorescent material. For rotating the objects the previous apparatus was taken over without modification except that the arc of rotation was shortened to run between 30° (apparent frontal-parallel) and 90° (line of regard.) (*See* Fig. 2.)

FIGURE 2



"Equivalent" shapes

Only these shapes corresponding to two angles of orientation were used, since considerable information as regards constancy throughout the tilt arc had already been gained. In addition, an Ames distorted room is not exactly an ideal surround for study of the tilt arc for its effect is to "compress" the arc through the induced anisotropic effect. Since the line of regard is not rotated whilst the frontal parallel is rotated the tilt arc will be 60° in one direction and 120° in the other.

The calculation of a comparable constancy value through the tilt arc depends upon the retention of a constant quantity for the stimulus value. Unfortunately this no longer remains the case with an Ames room, for with each move from the f/p plane the induced effect lessens until at the line of regard it is nil. This means that each stimulus object (corresponding to an irregular declination) would have a different stimulus value from its euclidean space value, but less than that induced by the equivalent space at the frontal parallel plane. The net result is to make calculations of stimulus values complex and hazardous. Unless the (S) and (R) values can be regarded with some confidence it is out of the question to compare constancy values.

Thus the possibilities of this experiment were restricted to—

1. Comparison of moving and stationary rotating outlines made compatible with the equivalent space.
2. Noting effect of varying velocity upon perception of such shapes.
3. Repeating observations using shapes calculated for normal euclidean space and incompatible with equivalent space.

The actual shapes used were given "S" values by calculation of expected change of shape and checked by use of control observers encouraged to match the shapes stationary, comparing the axes, etc., of the pair of shapes. These hypothetical "S" values were then used to calculate the Thouless ratios (not given here).

Description of Experiment

The experiment was carried out in an identical manner to those described above, except that it could be completed in a single session. The moving shapes were matched at 4 velocities of 15, 30, 45, and 60 seconds per sweep. The subject's aperture was arranged so that it was at one and the same time almost opposite the true centre of the near wall of the equivalent room, and practically opposite the centre of the rotating circle. This was placed inside one of the near windows of the room and about 1 metre from the observer. The fixed ellipse was to the left of the shape, separated from it by 15°. The subject stood before the apparatus in a darkened room and saw the interior by monocular vision. He recorded matches by means of a push-button and volunteered comments which were recorded on tape.

The shapes were first presented rotating at the four speeds in random order, after which they were set by hand under the subject's instructions. Six subjects were used, five of whom had taken part in earlier experiments.

Results of Experiment

A. *Outlines Calculated for Equivalent Space.*

Before dealing with the formal results, the general appearance of the space and reactions of the subjects deserve comment.

The general appearance of the structure was that of a room about three units deep by four units wide. If the "peep-hole" was moved to the left (nearest the long side) the room appeared narrower and deeper, with long narrow windows. The shapes seen through these windows changed in appearance also. This is an interesting result of the "induced" change of shape, since the ellipse on the left is some 15° from the centre of fixation (the circle) and turned 30° from frontal-parallel. Normally, such movement by the observer would cause the shape to appear wider (since it is then seen "head on" instead of to one side) yet the effect of the surround is such that with leftward movement, the longer (left) side is now taken as the reference side and the room becomes greater in depth and height than it is in width. The "windows" take part in these changes and appear to transmit them to the objects located within them, so that these now appear relatively narrower.

It was found that the addition of supplementary outlines such as wainscotes, floorboarding, picture rails, etc., strengthened the effect so these were added for the experiment proper. It must be admitted that when a little light from outside was allowed to fall upon the floor of the laboratory (seen through the "floor" of the

equivalent room) there was no apparent conflict between the "true" floor and the "equivalent" one, although the latter inclined at some 25°.

The full intensity of the induced effects take some seconds to develop. After gazing through the peephole for some time the subjects were asked for their comments on what they saw. All stated that they saw "a room with windows" which was "wider than it was long." It had "a floor and a ceiling" and in the windows were "a circle and an ellipse." Four subjects felt that "there was something very peculiar" about the room, but only two were able to say what this was unaided. These two subjects declared that the room "had no perspective" and that they "had no certain idea how big it was." The other subjects agreed fully with these suggestions in subsequent discussions, stating that they felt something like this, but were unable to "pin it down" or describe it spontaneously.

Various trials were made to test the power of the room to transmit induced changes of shape and size and to show that the total appearance was more than the simple correlate of the retinal image (this latter being simply the projection of a "normal" euclidean space). Thus a thin rod dipped in fluorescent dye was seen to change its apparent length when waved about in the space. A small toy motor-car similarly inserted was allowed to run along a board placed over the "floor" and appeared to change its shape and size when in different parts of the space. These trials showed that the induced effects might operate upon the rotating stimulus object.

When this was set in motion various matches were made at the several speeds. The subjects who had taken part in earlier experiments, all agreed that the appearance seemed the same as those previously experienced, the alternations between "turning round" and "being squeezed" occurring as in past experiments.

TABLE I

ROTATING SHAPES MATCHED IN EQUIVALENT SPACE AT FOUR VELOCITIES. SHAPES CALCULATED FOR THE SPACE. RESULTS AS DEGREES OF ARC

Subject	60 *sec.*		45 *sec.*		30 *sec.*		15 *sec.*	
	In	*Out*	*In*	*Out*	*In*	*Out*	*In*	*Out*
1 ..	27	28	32	32	31	31	36	37
2 ..	29	31	33	37	37	37	42	41
3 ..	31	30	27	27	27	28	32	31
4 ..	26	29	29	29	31	31	33	32
5 ..	21	22	22	23	22	23	26	25
6 ..	29	30	30	31	33	35	35	36
Mean ..	27·5	28	29	30	30	31	34	34
Excess ∠ ..	7·75°		9·5°		10·5°		14°	

Subjects 6. N = 384. Stimulus Value = 20°.

From Table I it may be seen that constancy rises with velocity as in earlier experiments, but that estimates of perceived shape for both stationary *and moving* shapes correspond with the characteristics of the equivalent space and not those of the euclidean space defined by the movement of the object. For euclidean space the S value of such a shape is 30°. That is to say, the comparison ellipse has an outline equal to a 30° projective declination of a circle, when seen from the right and subtending an angle of 15°. Yet the equivalent space gives a stimulus value match of

20° from the frontal-parallel plane. Thus there is an induced rotation of 10° and this persists even when the shape is rotating continuously. The fact that the curve of constancy rises smoothly with increase of speed as with earlier experiments tends to confirm that constancy is related to the "real" appearance of the shape *in terms of*

TABLE II

DISPERSIONS OF SCORES IN σW (DEGREES OF ANGLE) FOR SPACE ERRORS IN TABLE I

Subject	*Velocity. Seconds per sweep of 60°* 60 *sec.*	45 *sec.*	30 *sec.*	15 *sec.*
1 ..	1·0	1·12	3·4	1·22
2 ..	2·2	2·2	1·12	2·0
3 ..	1·0	0	1·22	1·4
4 ..	1·22	0·63	0·7	1·22
5 ..	1·5	1·8	1·6	2·55
6 ..	1·0	1·0	1·2	1·4
Mean ..	1·32	1·12	1·54	1·63

the surrounding equivalent space and *not* the space successively occupied by the outline itself. In fact, if the latter were true *the shapes could not be matched at all* since they are irregular in outline and their axes are not central. It will be seen, however (Table II) that space errors remain small and dispersion low, comparing favourably with earlier experiments. (Table I, Part I).

The range of velocities appears higher than in previous experiments. This is due merely to the shortening of the rotation arc from 90° to 60° whilst using the same speeds of rotation.

TABLE III

ROTATING SHAPE MATCHED IN EQUIVALENT SPACE AT FOUR VELOCITIES SHAPE CALCULATED FOR NORMAL SPACE. RESULTS AS DEGREES OF ARC

Subject	*Velocity. Seconds per sweep of 60°* 60 *sec.* *In*	60 *sec.* *Out*	45 *sec.* *In*	45 *sec.* *Out*	30 *sec.* *In*	30 *sec.* *Out*	15 *sec.* *In*	15 *sec.* *Out*
1 ..	36	39	48	42	43	47	44	48
2 ..	36	37	39	40	40	42	43	44
3 ..	37	39	37	42	41	45	53	48
4 ..	36	38	36	39	40	40	41	42
5 ..	39	41	40	43	41	45	45	49
6	36	39	41	43	.40	45	43	49
Mean	36·5	39	38·5	41·5	41	44	43·5	46·5
Excess ∠ ..	2·25°		5°		7·5°		10°	

Subject 6. N = 348. Stimulus Value = 35°.

B. Normal Space Outlines.

In part B of the experiment an ordinary circle and an ellipse corresponding to its projection at 40° (when seen from a similar viewpoint, subtending an angle 15° to the

left), were placed within the room and viewed in the same way as the first pair of shapes. Here the expectation was that with the shape stationary the effect of induced shape changes would have rendered matching impossible, the two shapes now appearing quite dissimilar. With the shape rotating it might have been expected that the effect of euclidean rotary motion would have been to restore the "true" shape and produce a match similar to that obtained in normal space.

In the event these expectations were only partially realized. The circle presented frontal-parallel was seen as an oblate ellipse. The ellipse in the true frontal-parallel was at an angle of 30° to the plane of the equivalent space and was seen as the projection of a circle at some 35° of inclination. It was possible to make matches with these figures (though with greater dispersion and variability in scoring), and the reason for this is probably the fact that induced effects tend to fall off as the line of regard is approached (as explained earlier) so that in turning the circle the effect of distortion is decreased.

When the circle was rotated continuously the same effects were seen as in Part A of the experiment. The match point is not that of normal space, but still one determined by the equivalent room.

TABLE IV

DISPERSIONS OF SCORES IN σW (DEGREES OF ANGLE) FOR SPACE ERRORS IN TABLE XII

Subject	Velocity. Seconds per sweep of 60°			
	60 sec.	45 sec.	30 sec.	15 sec.
1 ..	1·7	1·8	2·34	2·23
2 ..	1·12	1·8	2·12	2·0
3 ..	1·7	1·34	1·22	1·48
4 ..	2·2	1·12	1·0	1·58
5 ..	1·0	2·12	1·8	2·0
6 ..	1·87	3·0	1·0	1·12
Mean	1·6	1·86	1·58	1·74

Yet there is a noticeable change in that the shapes did become distinctly easier to compare, both subjectively and objectively. Subjects thought matching "very difficult" with the object stationary, but not very difficult when the circle rotated. This effect is confirmed when the dispersions are compared. (Tables II and IV.) Dispersions for stationary matching of the shapes is $\sigma w = 5{\cdot}29$ compared with σw 2·4 for stationary match of shapes calculated for equivalent space. The mean space errors for stationary matching of the two sets of shapes are 7° 30″ and 2° 30″. This indicates a considerable difference in matching conditions when compared with space errors and dispersions in the Tables I and III, and II and IV. An attempt was made to calculate Thouless's ratios for the two situations, but the results are not given since there appears little value in making any such quantitative comparison. It is enough to say that a degree of constancy is present in all the situations and increases with velocity of rotation.

Discussion

The results from this experiment are, though interesting, somewhat different from what was originally anticipated, and perhaps rather disappointing. It would seem that the degree of conflict between the rotating shape and the surrounding space was

insufficient to disorganize completely the set of appearances deriving from the equivalent space.

To some extent this might have been foreseen, firstly from the nature of the stimulus pattern. The circle is hardly an ideal shape for it has a very stable configuration due to lack of sharp changes in contour, absence of straight lines, etc. Second, the tilting of such a shape results in gentle and smooth alterations in appearance whilst at the same time lessening the induced effects.

Nevertheless, the experiment has confirmed that perception of a shape outline in depth, as distinct from projective or two-dimensional outline, takes place in accordance with the apparent properties of the spatial surround. Tilt or orientation is attributed to the object, guided in some way by the cues from the boundary outlines of the space.

As with perception in dark space or normal surrounds, intervention of rotary motion results in a qualitatively different situation. Thus when the shapes correspond to the requirements of equivalent space, they behave in an identical manner to an outline tilted in a normal euclidean space. This would seem an important conclusion when it is recalled that,

1. Perception takes place under conditions of monocular vision.
2. The peephole constitutes a reduction screen.
3. The outline has no surface.
4. Self luminosity does not vary with orientation of the object.
5. The space appears to lack "perspective" or any cues to "real" size.

With these points in mind it is interesting to note that for the perception of the stationary shape outline (Section A of the Results) on the basis of "snap" judgments a mean value of 28° 30″ is obtained—some 8° 30″ above the obtained stimulus value for a projective match in shape. This would suggest that surround cues play a considerable part in perception of orientation, and thereby, of shape. When to these considerations is added the fact that the comparisons are compatible only with the appearance of the outlines *in equivalent space*, it is difficult to resist the conclusion that spatial surround cues play the dominant rôle in establishing the frame of reference, in terms of which the actual stimuli are perceived. Comparison of the two shapes is not merely difficult in normal space, it is in fact impossible to match them at all.

In referring to the rôle of surround cues as establishing a frame of reference in terms of which the object is appreciated, it is not suggested that this is confined merely to producing a specific "anchorage point" or "sense of direction." This is a more limited function which has been discussed in the work of Asch & Witkin and has already been referred to. What is rather meant, when speaking of surround cues, is the function of *qualifying*, in an all-pervasive way, the spatial character of the object, and hence its perceived shape.

As regards the experiments described in Part II, had the expectation of visual conflict been completely fulfilled the following possibilities could have arisen.

1. No match possible with stationary shapes,/or
2. The match would be incompatible with the normal characteristics of the shapes,/or
3. It should have been incompatible with the character of the shapes perceived in equivalent space,/or
4. A match was obtained which could not be explained on any of these assumptions.

Whilst with moving shapes,

5. No match was possible,/or
6. A match occurs which, with increasing velocity is increasingly independent of the characteristics of the equivalent space,/or
7. Continues to correspond with the properties of the equivalent space,/or
8. Results in some compromise according to velocity and nature of stimulus,/or
9. Cannot be accounted for on any of the previous assumptions.

In the actual event, the results came closest to expectations (2), (7) and to some degree (8), whilst showing from the wider space errors and larger dispersions a degree of uncertainty in estimation of the second, as compared with the first type of shape. Despite this, however, there is a high correlation between the results in Tables I and III.

This paper brings to a conclusion the series of investigations concerned with shape constancy of rotating two-dimensional objects. For a comprehensive treatment of the general theory of shape perception, the perception of three-dimensional objects needs to be investigated. This involves the problem of orientation of the subject as distinct from rotation of the object, or in other words, the question of "point of view." A series of investigations dealing with this problem has recently been carried out by the present writer, which it is intended to summarize in the near future.

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THE EFFECT OF GOAL-SETTING AND ENCOURAGEMENT ON THE PERFORMANCE OF IMBECILE MEN

BY

N. O'CONNOR AND G. S. CLARIDGE

From the Medical Research Council Unit for Research in Occupational Adaptation, Maudsley Hospital, London.

Following previous work, three groups of imbecile men were asked to carry out a simple task over a period of nine trials. Self-competition (Goal) had previously improved the performance of one sub-group on this task significantly, and experiment showed that this improvement had been retained. By re-dividing the subjects, three groups were matched and asked to perform under different motivating conditions. These were: Control, Goal with Encouragement, and Goal with Indifference or neutrality shown by the investigator. Treatment of the results was based on the slope of regression lines. It was found that encouragement had a significant effect on the performance of subjects working under goal conditions, but that goal conditions without encouragement did not significantly improve performance over control conditions.

I

Introduction

This investigation follows from work reported elsewhere by Gordon, O'Connor and Tizard (1954; 1955). The previous studies concerned the effectiveness of goal-setting as an incentive force for male imbeciles on two tasks, one a test of endurance (1954) and the other a manual repetitive task lasting one hour (1955). Both investigations established the superiority of the goal-setting technique to the incentive effect of individual or group competition, and to control conditions.

II

Object of the Investigation

In the previous studies which were carried out by Gordon, it is possible that the success of his Goal Group depended to some extent on his personal approach to their goal-striving and success or failure and not simply on their own self-competition as an internalized drive.

The aim of the present experiment was to make a more clear-cut distinction between encouragement and goal-striving and to discover their relative importance in the motivation of subjects performing under conditions of Self-Competition. As the same subjects and the same task were used as with the previous experiments, a further subsidiary aim of the investigation was to discover how far the skill, learned during previous work almost a year before, had been retained during the interval. To bring out the significance of recognized achievement and unrecognized self-competition, a distinction was made between "Goal with Encouragement" and "Goal with Indifference." In the first case achievement and failure was always sympathetically observed by the investigator and with the latter success or failure in goal attainment were alike treated with flat indifference.

III

Brief Note on the Task

The task used was the Nail Frame test, devised by Gordon (1953). It consisted briefly of the following: a frame (8 in. × 10 in.) was covered with a sheet of perforated zinc (82 holes to the sq. in.), in which a column consisted of 60 holes exactly. The subjects were asked to insert small scutcheon pins into the holes, working vertically down a column and completing it before starting on the next.

IV

Design of Experiment and Procedure

Three groups of subjects were needed for the experiment. These were called: (i) Goal with Encouragement Group, (ii) Goal with Indifference Group, (iii) Control Group. The forty male imbeciles employed had already worked on the task a year before. Therefore, as the groups were to be matched on performance level, the first two trials of the experiment served not only as a basis for matching, but also as a measure of retention of the skill. Thus, for the first two days, the subjects were taken in the groups to which they were originally allotted by Gordon, i.e. four groups of ten subjects, referred to as Goal, Competition, Co-operation and Control Groups. (In the previous work "Competition" has the meaning individual competition, and "Co-operation" the meaning competition between groups.) Two groups were taken in the morning and two in the afternoon. Each group worked for an hour, this constituting a trial.

At the beginning of the first trial the procedure to be followed was briefly sketched, but this was found to be unnecessary, as all subjects remembered what to do. Each of the four groups was told to work as hard as they could and at the end of the trial they were told, "You worked very well. I'll see you all again to-morrow at the same time." On the second day exactly the same procedure was followed. The larger of the scores for each subject was used as a basis for forming three matched groups. All forty subjects were ranked and split off into matched triplets, a member of each triplet being assigned at random to one of the three groups. This resulted in three groups of thirteen subjects, the remaining fortieth subject not being used.

For the next ten days the subjects worked under one of the following conditions.

9.30 *a.m. Goal with Encouragement Group* (13 *subjects*). A red nail was inserted at a point 2 per cent. higher than the subject's previous best score; this indicated his goal for the day. This was modified after the third trial to a point 1 per cent. higher than the best score in the previous three trials, for it was found that few were succeeding in reaching their goals.

At the beginning of each trial the subjects were told: "See if you can all get your goals to-day. You worked very well yesterday, but see if you can work even harder to-day." Half-way through the hourly period they were told: "Come on, boys, keep working hard. You are doing very well at the moment. Let's see you get your goals." At the end of each trial those who succeeded in reaching their goals were told: "That was very good; see if you can get your goal again to-morrow." Those who failed were told: "Bad luck; you did very well anyway and you'll get it to-morrow." At intervals during the session the experimenter walked round nodding approval of work to individual boys. The intervals were standardized, but an element of difference occurred in the treatment of individuals in so far as the experimenter's remarks were addressed to the group in general, but his direct attention might be focussed on one person rather than another.

For the last four trials encouragement of the kind given at half-time was given every quarter of an hour. This was done to increase the effectiveness of the encouragement.

11 *a.m. Goal with Indifference Group* (13 *subjects*). The goal was calculated and indicated exactly as for the Goal with Encouragement Group, with the same change after the third trial.

At the beginning of each trial the subjects were told: "See if you can get your goals to-day." At the end they were told: "You worked well to-day. I'll see you all again to-morrow at the same time." No comment was made about success or failure to reach the goal, and as far as possible indifference was shown when a boy came with obvious delight to tell the experimenter that he had reached his goal. Throughout the experimental period no comment was made except to keep the subjects working during interruptions.

1.30 *p.m.* *Control Group* (13 *subjects*). At the beginning of each trial the subjects were told: "Work as hard as you can," and at the end, "You worked well to-day; I'll see you again to-morrow at the same time."

At the beginning of the ninth trial the subjects in all groups were told that they would be paid at the end of the experiment. This was done because they had been paid in their previous work with Gordon, and several subjects enquired during the course of the experiment whether they were to receive money. Some even became quite confident that they would. Before trial 9, however, any questions by them regarding payment were answered in a non-commital way to avoid affecting motivation.

V

Results

1. *Retention.* On the first day all groups settled down to work surprisingly quickly, considering the interval which had elapsed since they had done this work. All subjects remembered what they had to do, although there were one or two deviations from the prescribed technique. These were corrected as were any variations that appeared during the course of the experiment. Distraction was unavoidable at the beginning of the first day, but subjects were soon working quietly and industriously and on the whole this persisted throughout the investigation.

TABLE I

Mean Score for Each Group on Each Trial

"A" Matching (Retention) Trials

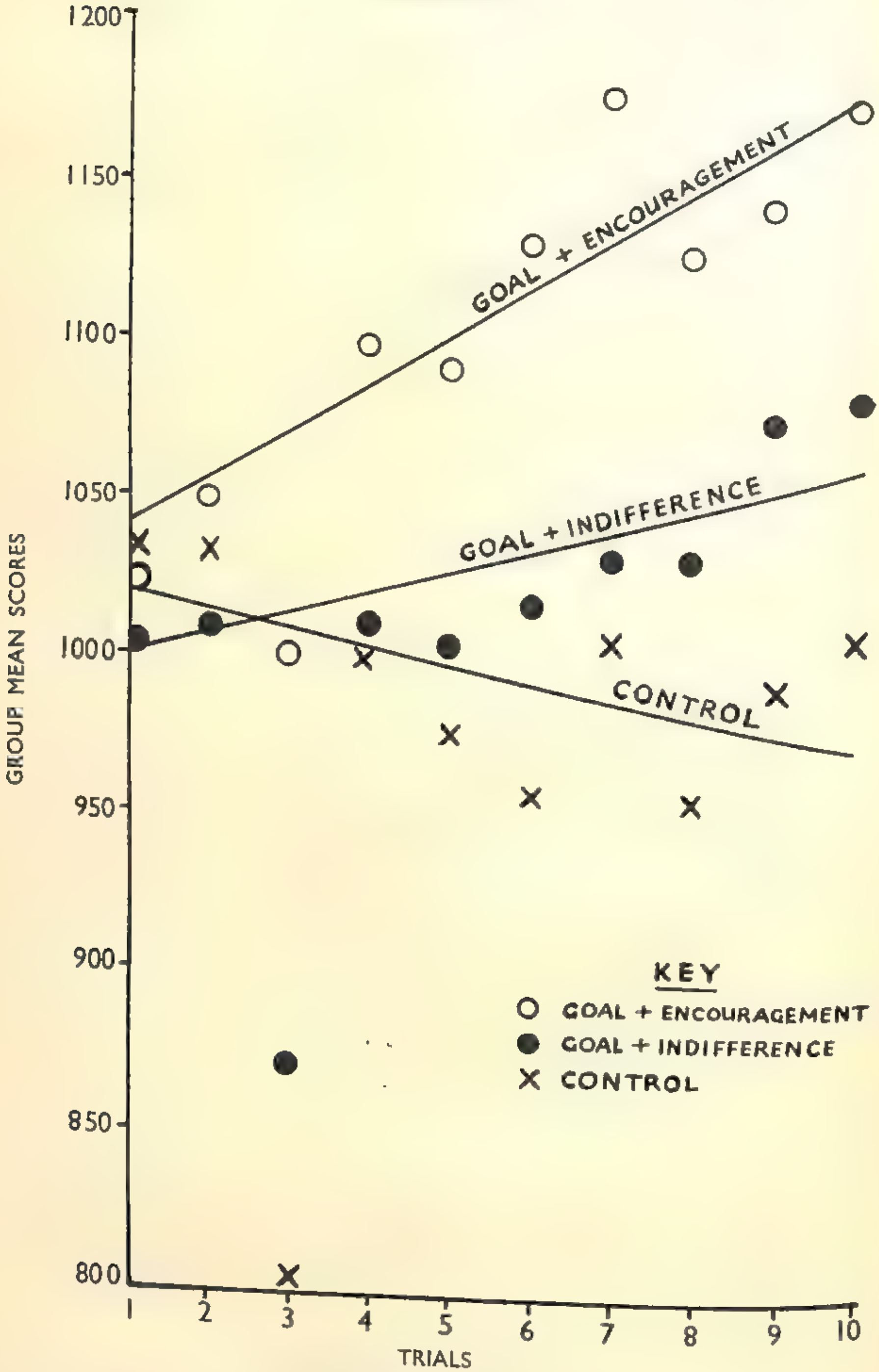
Trial	*Goal*	*Co-operation*	*Competition*	*Control*
1	1,037	1,030	998	930
2	1,138	1,059	981	945

"B" Experimental Conditions

Trial	*Goal and Encouragement*	*Goal and Indifference*	*Control*
1	1,019	1,003	1,030
2	1,048	1,010	1,029
3	1,001	868	795
4	1,099	1,007	996
5	1,092	1,004	974
6	1,133	1,013	956
7	1,172	1,028	1,010
8	1,133	1,028	952
9	1,140	1,071	989
10	1,170	1,074	1,003

The group means (Table I "A") for the two retention trials indicate that the Goal Group retained its superiority and that the Control Group's performance was lowest. The "value of *t*" for the difference was 2·4309, which is significant at the 0·05 level of confidence. Other *t's* were not significant (Co-operation vs. Control 1·306; Competition vs. Control 0·486). The Co-operation and Competition Groups were intermediate—in that order. It is interesting to note the reversal in order of these

FIGURE 1



Group Means and Regression Lines.

two groups; for Gordon found that his Competition Group performed better than the Co-operation Group, both while working under incentive conditions and during his own retention trials.

Consideration of individual scores indicates that on the whole subjects tended to return to roughly the performance level reached on the 30th trial of Gordon's experiment, which lasted for 42 trials. As exceptions to this, there were a few cases where the return was to a level of achievement approximating the 10th trial. Only the Competition Group failed to increase its mean score on the second retention trial.

2. *Main experimental trials.* Table I "B" shows the group means for the ten trials under experimental conditions. The figures are plotted as a graph in Figure 1.

It is apparent from Figure 1 that on the third trial an extreme drop in the performance level of all groups took place. This was almost certainly due to the

TABLE II

ANALYSIS OF VARIANCE TABLE

Source	*Sum of squares*	*d.f.*	*Variance estimate*	*F-ratios*	*Level of signif.*
Between groups ..	8,712·666	2	4,356·333	5·4835	0·05
Between matchings ..	155,398·849	12	1,294·990	1·6301	Not signif.
Between trials	1,668·615	8	208·576	1·1900	Not signif.
Interaction: groups × trials	2,804·308	16	175·269	2·9546	0·001
Interaction: groups × matchings	19,066·741	24	794·447	13·3925	0·001
Interaction: trials × matchings	6,436·792	96	67·049	1·302	Not signif.
Interaction: trials × matchings × groups (residual)	11,389·618	192			
Total	205,477·589	350			

Note.—Where interactions are significant, the significance of a main effect has, of course, to be treated against the largest relevant interaction.

fact that at the beginning of the third day a new supply of scutcheon pins was mixed with the ones already in use. The new pins were of a shiny brass colour (the old ones being dullish in appearance) and had slightly larger heads. It is possible that there were some distracting effect from the contrast in appearance of the two types of pins, as was evident in the case of one boy who was obviously selecting pins of one kind. Further interference may have arisen because of the larger sized head which made it difficult for the subject to insert adjacent pins. Observation of work by the experimenter as well as complaints by several subjects confirmed this. Although unfortunate in some ways, this sudden drop raises the interesting question of the dependence of performance level on the maintenance of a standard routine in tasks of this kind, especially, one might expect, with the type of subject used. It is noticeable, however, that the decrease in performance level of the three groups

TABLE III

LINEAR REGRESSION COEFFICIENTS

Group	*Subject*	*Average increase per trial* (a)	*Performance level* (b)
Goal with Encouragement ..	1	12·5	1,440
	2	15·0	1,296
	3	18·7	1,153
	4	15·7	1,098
	5	2·1	1,279
	6	19·3	1,037
	7	3·4	1,137
	8	17·1	796
	9	36·3	924
	10	12·6	893
	11	2·7	897
	12	4·3	737
	13	25·1	676
		Mean 14·2	
Goal with Indifference	1	16·2	1,373
	2	−3·2	1,154
	3	5·1	1,233
	4	10·0	1,174
	5	10·3	959
	6	22·5	1,077
	7	10·1	910
	8	−2·1	928
	9	17·5	976
	10	−8·9	778
	11	1·3	778
	12	1·5	838
	13	4·2	741
		Mean 6·5	
Control	1	5·9	1,484
	2	−13·1	1,428
	3	−7·2	1,244
	4	13·8	1,105
	5	0·77	939
	6	−5·4	947
	7	−7·6	815
	8	−17·9	1,071
	9	−30·0	709
	10	−15·9	892
	11	−0·24	876
	12	2·0	837
	13	−48·7	935
		Mean −4·9	

was in the order expected, those who were presumably most motivated (the Goal with Encouragement Group) overcoming the interference best. Following a return to one kind of pin on the next trial, all groups increased in level, again in the predicted order. Since the third trial was not equivalent to the other nine trials, the data

obtained from it were not used in the further analysis of results. Systematic differences between groups, between trials and between matchings (i.e. between individuals of equivalent performance level, irrespective of group) and their interactions were isolated by a three-way Analysis of Variance, the appropriate F-ratios and levels of significance being shown in Table II. The fact that the interactions between Groups and Trials and between Groups and Matchings were significant showed that the groups improved at significantly different rates, and that the inter-trial matchings were disturbed by this. This led us to consider the rate of improvement in performance of each individual subject. Regression lines were fitted to each individual's scores for the nine trials (as noted above, the third trial was omitted). Inspection confirmed that linear regressions provided a good fit. The scatter about the regression lines was shown to be small, the standard deviation being about 80 score units. The regression coefficients for linear regression (score = a (trials) + b) are shown in Table III, together with the group means of the coefficient representing the average rate of increase.

GOAL with ENCOURAGEMENT	14·2
GOAL with INDIFFERENCE	6·5
CONTROL	−4·9

It is clear that subjects performing under Goal with Encouragement conditions improved on the average at a greater rate than those working towards a goal without encouragement. Control subjects, that is those with whom no incentive was used, tended to show a progressive decrease in performance level.

Groups by Matchings being a significant interaction showed that the three group effects varied according to the initial, matched level of the individual in each group. Thus the individual rates of increase in Table III show that no subject failed to improve in the Goal with Encouragement Group, whilst three in the Goal with Indifference Group and nine in the Control failed to show any improvement during the experiment.

In particular, the lower matchings improved in the Goal with Encouragement Group but not in the Goal with Indifference, or the Control Group. The higher matchings improved in the two Goal Groups but did not in the Control Group. It should be noted that the average slope of the Control Group is excessively depressed by the inclusion of the somewhat unusual thirteenth member of the Control Group; if his score is excluded the mean slope of this group is not −4·9 but −1·24, or nearly zero.

One point emerges clearly, namely, that the differentiation of the group trends is only made possible by the original careful matching. Any individual's total improvement in the Goal Group is only 140 points and the difference in score between the first and last individual in that group is in the region of 700. To have merely randomized might well have obscured the learning trend difference. The interaction term in an Analysis of Variance would have been at least ten times greater in such circumstances.

Finally, to estimate the significance of the difference between rates of improvement of the three groups taken separately, the t-test for related means was applied, the relevant regression coefficients (Table III) being used for this purpose. The following values for "t" were found:

Between Goal + Encouragement and Control = 3·3621 (Significant at 0·01 level).
Between Goal + Encouragement and Goal + Indifference = 2·4539 (Significant at 0·05 level)
Between Goal + Indifference and Control = 0·2366 (Not significant)

These results indicate that goal-striving in itself does not increase performance level significantly unless accompanied by encouragement and social approval of work. This suggests a comparison with Gordon's previous results with the same subjects. However, a comparison cannot strictly be made, because, although a year elapsed between the two experiments, the subjects were taken, in the present study at a different stage of performance, i.e. after practice had had its initial effect. Further, although Gordon did not make explicit use of encouragement during the working period, he gave approval of work at the end of each session. His Goal group is therefore not comparable with our Goal with Indifference group. It may in fact be the case that encouragement need not be made explicit in an incentive situation of this kind.

VI

Conclusions

It was concluded that the effect of self-competition as an incentive for increasing the performance level of imbecile subjects on a repetitive motor task was found to be largely due to factors other than those associated with goal-striving as such.

Specific standards are certainly valuable in systematically increasing performance levels, as Mace (1935) has shown, but the complexity of secondary motivating factors, which many previous writers, such as Leuba (1932), have mentioned, must also be considered carefully in any experimental situation. The results of this experiment reflect a situation in which social approval appears to be of primary importance. Many previous studies of motivation have depended upon work with rats, where Drive, Habit Strength and Inhibition appear to be adequate explanatory variables. A number of studies with humans have shown the inadequacy of these simple concepts in explaining human learning and performance, e.g. those of Abel (1938) and Harlow (1950), but much remains to be done to specify the multiple social incentives which appear to be operating, even with imbeciles. McClelland *et al.* (1953) have recently given some indication of the possibilities in this field.

A second observation can be made concerning the retention trials in the present experiment: the theoretically consistent retention shown over a period of one year since Gordon's earlier work provides somewhat unusual evidence of the relatively long-term effects of an external incentive. Such a finding may have implications similar to Hebb's (1952) experiments, and may demonstrate the importance of good incentive conditions in early stages of training.

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MISCELLANEA

A NOTE ON BINAURAL FUSION

BY

D. E. BROADBENT

From the Medical Research Council Applied Psychology Unit, Cambridge.

It is a psychological commonplace to say that information coming from one eye may be fused with different information from the other eye to give a single visual world. In hearing, however, it is usual for the stimuli reaching the two ears to be closely similar. Differences in intensity or time of arrival at the ears may indeed produce a perception of localisation (Stevens & Newman, 1936) or the rather different precedence effect used in stereophonic reproduction (Wallach, Newman & Rosenzweig, 1949); but there is little reported comparable to, say, the filling up of the blind spot in one eye by information received through the other. The tendency of recent research has been in fact (Cherry, 1953; Broadbent, 1954) to emphasise the cases in which, when different stimuli arrive at the two ears, the information on one ear is lost more or less completely while the other ear controls response.

The writer observed recently, however, that if a speaking voice is heard through two head-phones, with a filter eliminating the low frequency components on one ear and another eliminating the high frequency components on the other ear, a considerable degree of fusion is found. Eighteen research workers have been used as observers, four of them being previously aware of the nature of the stimulus.

Recorded speech was used to avoid visual suggestion. The low-pass filter was set at 450 c.p.s. and the high-pass at 2000 c.p.s. Both attenuated at about 18 db per octave so that a considerable gap was left in the spectrum between the signals on the two ears. This arrangement was designed to oppose fusion as much as possible, but when asked whether they heard one voice or two, fourteen observers said one. Of the remaining four: one said "I could convince myself that it was one, but there's something funny about it." Another retracted the previous judgment on hearing a genuine two-voice presentation and a third reported two voices even when the stimuli on the two ears were the same. The proportion of fusions was the same for both naive and sophisticated observers.

Fourteen of the subjects were presented with a further series of sounds, immediately after hearing the speech record. These sounds were: (*a*) a metronome, fed simultaneously through the high-pass filter to one ear and through the low pass filter to the other ear. (*b*) A speech recording, fed through the high-pass filter to one ear, and to the other ear through the low-pass filter after a time-lag of about 0·25 sec. This lag would have produced a perception of two voices even without filtering. (*c*) A pure tone of 3000 c.p.s. to one ear and another of 500 c.p.s. to the other ear, no filters being involved in either channel. (*d*) A record of a male voice intoning a steady vowel sound delivered to one ear through the high-pass filter. Another copy of the same recording was delivered to the other ear through the low-pass filter, the two records being started and stopped independently. The vowel used was the *i* (as in 'seen') of the International Phonetic Alphabet, since this vowel has a particularly large gap between the first and second resonances (Potter, Kopp & Green, 1947). (*e*) The same vowel sound, played to one ear through the low-pass filter and the other through the high-pass filter: the same record was used for both channels in this case and the original time relations were therefore preserved. Finally (*f*) thirteen of the subjects were informed of the nature of the stimuli and asked to say which ear was receiving the low-pitched stimulus from a sample of speech treated in the original fashion.

All but three subjects heard (*a*) as a single sound: all heard (*b*) and (*c*) as two separate sounds at the two ears. Eight subjects fused (*d*) and ten (*e*). Completely chance results were obtained on the final question, six subjects choosing one ear and seven the other. It is particularly striking that even the four subjects who said they heard two voices gave chance results on this question, two picking one ear and two the other. No subject made an error in assigning the pure tones of (*c*) to the correct ears.

These chance results perhaps establish the fusion more convincingly than the reported judgments on whether one voice or two were heard: but the differences in the number of

fused judgments between sounds (*b*) and (*c*) on the one hand, and the metronome and voice sounds on the other are of course highly significant statistically. The nature of the sounds which are or are not fused suggests that the important factor in producing fusion is the temporal relation between the arrival of stimuli at the two ears. In the crude sense, obviously each consonant of a speech message occurs simultaneously in the two channels, even though the frequencies passed are different, and the same relationship is true of a metronome; but not of asynchronous speech or of pure tones. But the fusion of an intoned vowel makes it clear that some further factor is operative. This may perhaps be sought in the similarity of the envelope wave-form of the voice sounds, which will be the same on both channels even though the Fourier analysis is different: compare, for example, the vertical striations which appear in speech spectrograms at an interval varying with the basic pitch of the voice (Potter, Kopp & Green, 1947). That the ear should make use of envelope wave-forms rather than Fourier components in complex sounds was of course quite contrary to the early resonance theory of hearing: but there is much contemporary evidence that it does do so (Mathes & Miller, 1947; Davis, 1952; Davis, Silverman & McAuliffe, 1951). The present observation joins this evidence.

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PROCEEDINGS OF THE EXPERIMENTAL PSYCHOLOGY GROUP, 1954

8th–9th April, 1954. Extended Meeting at Oxford. *1st Session:* Short communications by members of the Institute of Experimental Psychology—I. Howarth and J. S. Sutherland (both by invitation). "Some Neurological Aspects of Attention," by C. W. M. Whitty (by invitation). *2nd Session:* "The Organization of the Cerebral Cortex," by D. A. Sholl (by invitation). *3rd Session:* Demonstration of Apparatus by J. A. Deutsch. Short communication by a member of the Institute of Experimental Psychology—J. Langdon (by invitation).

6th–8th July, 1954. Extended Meeting at Cambridge. *1st Session:* "An Experiment on Adaptation to Level of Difficulty", by A. W. Heim. "A Note on Auditory Imperception," by D. Russell Davis. *2nd Session:* Symposium on "The Place of Psychophysics in Modern Science," by A. D. Harris, P. H. R. James, A. R. Jonckheere and R. L. Gregory. *3rd Session:* Symposium (continued). Discussion opened by L. S. Penrose (by invitation). *4th Session:* "Human Relations and Automatic Control," by P. Fensham (by invitation). "Some New Problems in Tactual Perception," by O. L. Zangwill.

4th–5th January, 1955. 8th Annual General Meeting at University College, London. *1st Session:* "The Effect of Time on Simple Operant Conditioning and Extinction," by V. R. Cane. *2nd Session:* "An Inquiry into the Effect of Prematurity on Psychological and Physical Development," by I. Heasman (by invitation). "The Role of Probability Models in the Study of Learning," by R. J. Audley (by invitation). "Loss of Information from Precis Compared with their Source," by P. C. Wason (by invitation). *3rd Session:* "A Study of Problem Solving," by E. Rayner (by invitation). "Some Effects of Vitamin B_1 Deprivation and Caloric Deficiency on Behaviour under Stress and on Learning," by F. Knöpfelmacher (by invitation). *4th Session:* "An Experimental Study of Luneberg's Theory of Space Perception," by A. Zajackowska (by invitation). "The Effects of Perceptual Isolation," by T. H. Scott (by invitation).

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DIFFERENCE-LIMENS FOR PHOTIC INTERMITTENCE *

BY

J. W. GEBHARD, G. H. MOWBRAY and C. L. BYHAM

From the Johns Hopkins University, Applied Physics Laboratory, Silver Spring, Maryland

Difference-limens for visual intermittence were measured in the range of one to 45 cps. The standard deviations obtained varied from 0·02 to 0·41 cps. and the relative difference-limens, $\frac{\Delta f}{f}$, computed from the average deviations, were from 0·005 to 0·024. This surprising capacity of the eye to react to differences in intermittence resulted in 375 just noticeable differences in a range of only 45 cps.

I

Introduction

The temporal resolving power of the eye has received little attention, except for the bearing that the critical flicker frequency (CFF) has on this problem. The ear, however, is considered to be the time analyser *par excellence* and, accordingly, has been extensively studied (Stevens and Davis, 1938). Mowbray and Gebhard (1954) recently examined the temporal acuity of the eye in a new way with surprising results. Difference-limens (DL) for intermittent white light were studied in the range of one to 45 cps. and were shown always to be smaller than 0·8 cps. When the DLs shown in Figure 1 were integrated, the number of just noticeable differences (j.n.d.) reached the large sum of 280. This appeared to be a very creditable performance for the eye. The present study extends the data on DLs for flicker. In the experiment previously reported, only 20 to 50 measurements per frequency for each of two subjects were obtained. Now, using two new subjects, the number of thresholds obtained has been increased to 150. The stimulus conditions and apparatus were exactly the same, so the results of the two experiments can be directly compared.

II

Method

Apparatus.

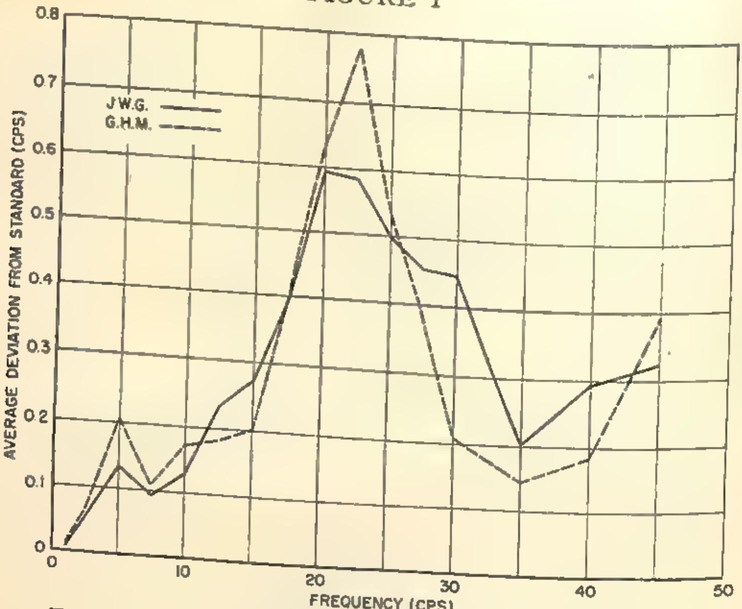
The flickering stimulus light was produced by a single Sylvania R1131C glow-modulator tube whose input was an electric square wave. Two separate and independent variable-frequency generators furnished the square-wave oscillations for the standard and the

* This report was prepared under Contract NOrd 7386 between the Bureau of Ordnance, U.S. Navy and The Johns Hopkins University and was read at the 35th Meeting of the Armed Forces-National Research Council Vision Committee, 3rd to 5th November, 1954, Toronto, Canada.

comparison channels. The amplitude of the output wave forms of these two channels were equated by means of oscilliscope measurements. Frequency measurements were accomplished by a Hewlett-Packard electronic timer, Model 522B.

The Sylvania glow-modulator tube used provided a light output that varied essentially in a linear manner with the current passing through it. It was of the hollow cathode or crater type, and a high ionization density was obtained, which, together with the fact that the discharge was viewed in depth, provided a high effective intensity. The tube had an "on" and an "off" time of about 7–10 microseconds and could be modulated at above 15,000 cps. The operation of the glow-modulator tube was completely silent at all times.

FIGURE 1



The A.D. of the thresholds as a function of frequency. Mowbray and Gebhard (1954).

The standard and comparison input channels were separately applied to the tube by a three position, silent-switch controlled by the subject. The central position of the switch was an "off" position so that in going from either standard to comparison or comparison to standard an intermediate "off" position had to be negotiated. The transition could be made as rapidly or slowly as desired, but overlap of the standard and comparison channels was impossible. Since both input channels were operative at all times, the change-over could be essentially instantaneous. However, the rapidity with which the change-over was accomplished was left to the discretion of the subject, as was the amount of time spent observing either channel.

Adjustment of the frequency of the comparison channel was made by the subject using a linear helical potentiometer.

The flickering stimulus was viewed binocularly on a 1° circular spot with a homogeneous luminance of 98 mL. The spot was centred in a white surround of 71° subtense held at 44 mL.

Procedure.

For each frequency at which judgments were made, the standard channel was carefully adjusted, and the potentiometer controlling the frequency of the comparison channel was set some indeterminate distance either above or below the standard frequency. The subject then adjusted the comparison channel until he was satisfied that the two frequencies were equal. He was allowed to move the control in one direction only. In the event that he felt he had passed the standard frequency, he was allowed to repeat the trial. When the subject was satisfied that he had obtained a match between the

standard and comparison frequencies, a measurement of the comparison frequency was made with the electronic timer and the reading recorded. The accuracy of the measurements was 0·5 per cent.

For each replication of the experiment, 16 frequencies were presented to two subjects in random order. At each frequency the subject made five measures at a sitting, alternating between ascending and descending thresholds. He then changed with the other subject until each had made 15 matches. There were 10 complete replications, resulting in 150 matches per frequency for each subject and a total of 4,800 thresholds for the entire experiment. The experimental design, therefore, permitted the data to be divided into as many as 10 equal parts.

III

Treatment of Data and Results

Normality of Threshold Data.

The first matter of interest in this experiment was the distribution of the individual thresholds about each standard frequency. This was investigated by combining both subjects and all 10 replications. The normal probability distribution was fitted to each of the 16 frequency distributions, and the χ^2 test of goodness of fit was applied. The tests indicated the surprising fact that every one of these distributions was non-normal. The reason for the non-normality was determined by graphic investigation to be extreme leptokurtosis. This is to say that a larger proportion of thresholds than would be predicted by the normal distribution fell close to the standard frequency. There was no evidence at any frequency of statistically significant skewness.

Because the subjects were known to have been learning rapidly, it was felt that this could have caused leptokurtosis by introducing a few extreme errors early in the experiment and a large proportion of relatively small errors in the later stages. When the experiment was divided into two equal parts, and the second five replications were examined separately, this supposition was supported in that the leptokurtosis at some frequencies was reduced. Further analysis, therefore, was based entirely on the combined judgments of both subjects in the second half of the experiment. These samples of 150 measures per frequency were still leptokurtic, although at 11 of the 16 frequencies there was a probability ranging from 0·1 to 0·9 that the experimental sample had been drawn from a normal population. The remaining five frequencies of 1, 5, 30, 40 and 45 cps. were still found to be inconsistent with the hypothesis of normality. A more valid hypothesis, therefore, is that all of these distributions are leptokurtic. This implies that the eye is able to succeed better at establishing relative thresholds than would have been expected on the basis of the theory of random errors of measurement.

Effect of Practice.

Since learning is an important factor in psychophysical experiments, the standard deviation (S.D.) of the individual measures was computed for each subject as each replication was completed. It was noted that considerable improvement was taking place during the early replications. As the experiment progressed, the subjects gradually became less erratic, and their thresholds became smaller.

At the completion of the experiment, all replications were combined for each subject and the S.Ds. computed. When plotted against frequency, these thresholds approximate the pattern anticipated on the basis of the data previously collected and shown in Figure 1. Nevertheless, the two subjects were seen to be very variable with all data considered.

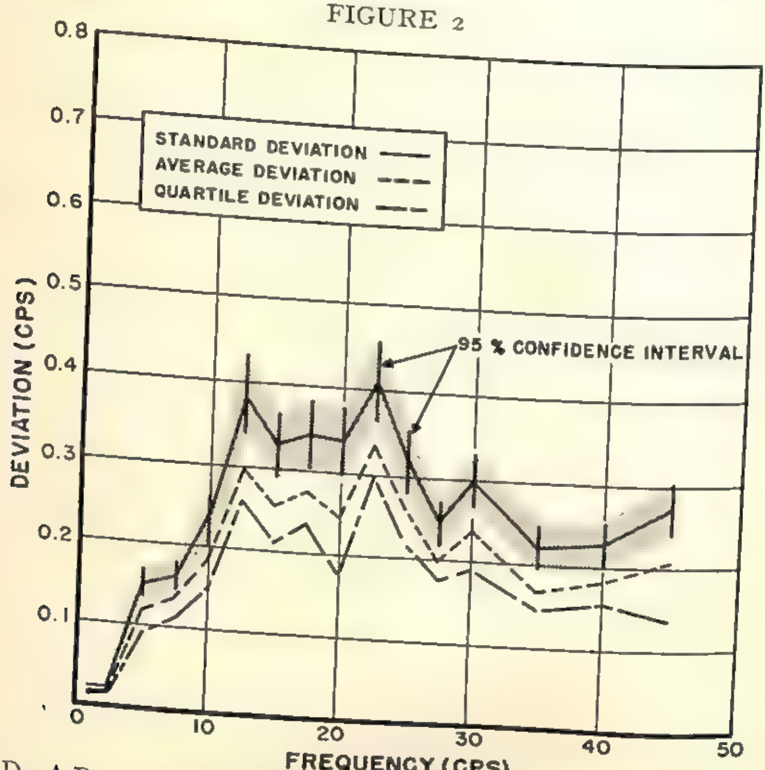
When the experiment was divided into two equal parts, however, the thresholds

in the first half were found to be very high. Both subjects were extremely variable, and their peaks and troughs fell at different frequencies. The second half of the experiment showed the thresholds to be much lower than in the first half. After five replications of practice, both subjects had become more stable, and their agreement was rather good.

The DL function.

The combined data of the two subjects for the second half of the experiment presents the reasonably stable form of the thresholds shown in Figure 2. While

FIGURE 2



S.D., A.D. and quartile deviations of the thresholds as a function of frequency for the second five replications. Subjects are combined, and the 95 per cent. confidence interval is shown about the S.D.

a peak is seen at 12·5 cps. and 30 cps., the 95 per cent. confidence intervals placed around the sample S.Ds. indicate that the population values are still not at all well defined with the amount of data collected. Average deviations (A.D.) and quartile deviations have also been computed for the second half of the experiment and are plotted in Figure 2 to show the patterns of these alternative measures of threshold. For the stimulus conditions of both this and the previous experiments, a comparison of Figures 1 and 2 shows that the DLs rise sharply between one and about 15 cps., reach a maximum at between 20 and 25 cps., fall somewhat in the region of 30 to 35 cps. and finally rise again as the fusion point of about 50 cps. is approached. The more highly practiced subjects of the second experiment produced lower thresholds in the middle range of frequencies but appear not to have altered the general shape of the function originally reported.

Integration of DLs.

The next step was to determine the number of j.n.ds. for the most stable part of the data. This involved the graphic integration of $\frac{1}{\Delta f}$, where Δf was the A.D. of the measures obtained at each frequency in the second half of the experiment. The number of j.n.ds. between one and 45 cps. is 375, a total considerably higher than the 280 reported earlier.

Summary of results.

The important findings of the second five replications of the experiment are brought together in Table I. The accuracy with which the standard frequency is measured

TABLE I

SUMMARY OF RESULTS FOR SECOND FIVE REPLICATIONS. BOTH SUBJECTS COMBINED.

Standard frequency (cps.)	*Mean measured frequency (cps.)*	*Mean difference (cps.)*	*S.D. (cps.)*	*A.D. (cps.)*	*$\frac{\Delta f}{f}$ computed from A.D.*	*Cumulative DLs*
1	1·002	0·002	0·022	0·015	0·015	50
2·5	2·498	0·002	0·022	0·017	0·007	168
5	5·046	0·046	0·151	0·118	0·024	189
7·5	7·480	0·020	0·161	0·132	0·018	208
10	9·991	0·009	0·238	0·184	0·018	222
12·5	12·531	0·031	0·388	0·299	0·024	230
15	15·011	0·011	0·329	0·254	0·017	240
17·5	17·522	0·022	0·343	0·275	0·016	249
20	19·969	0·031	0·338	0·246	0·012	259
22·5	22·504	0·004	0·411	0·340	0·015	266
25	25·018	0·018	0·320	0·261	0·010	276
27·5	27·501	0·001	0·246	0·197	0·007	289
30	29·909	0·091	0·301	0·238	0·008	305
35	34·986	0·014	0·221	0·166	0·005	335
40	39·928	0·072	0·227	0·181	0·005	363
45	44·957	0·043	0·273	0·206	0·005	375

is shown in column 3. It will be noted that the obtained frequencies differ on the average from the standard by never more than 0·09 cps., and 12 of the value are under 0·031 cps. In column 6, the relative DL, $\frac{\Delta f}{f}$, is shown. The range is from 0·005 to 0·024 and is somewhat smaller than was obtained in the first experiment. Column 7 shows the cumulated DLs.

IV

DISCUSSION

Critical flicker frequency vs. critical flutter frequency.

Studies on the CFF do not reveal the eye as a strong competitor of the ear for honours in speed of response. The highest frequency at which interrupted white light still appears to flicker is only about 60 cps. under optimum conditions. This frequency represents a period of 16 msec. On the other hand, the ear is able to discriminate interrupted white noise, or flutter as it has been termed by Miller and Taylor (1948), at over 1000 cps. This is a period of less than one msec.

Difference-limens.

Another way of looking at the temporal acuity of the eye or ear is the DL method chosen for this experiment. The smallest increment in frequency that produces a j.n.d. in experience measures an important aspect of the temporal resolution of the eye or ear. The differential sensitivity to the frequency of a tone has been reported in many studies on pitch discrimination (Stevens and Davis, 1938). Recently, Miller and Taylor (1948) measured DLs for flutter. Table II shows the relative DLs for pitch, flutter and flicker equated as well as possible by selecting from the available data.

TABLE II

Comparison of $\Delta f/f$ for Pitch, Auditory Flutter and Photic Flicker

Sensation[1] *level (db.)*	*Pitch*[2]	*Flutter*[3]		*Flicker*[4]	*Flicker*[5]
	50	50	100	85	85
frequency (cps.)					
20					
40		0·100	0·100	0·031	0·012
62		0·150	0·075	0·006	0·005
80	0·0351				
120		0·162	0·075		
125		0·208	0·083		
160	0·0270				
240		0·250	0·094		
250		0·333	0·217		
320	0·0099				
500		0·459	0·459		
1,000	0·0042				
2,000	0·0036				
4,000	0·0019				
8,000	0·0023				
11,700	0·0025				
	0·0030				

[1] In audition the sensation level is well understood to be the intensity level of a sound expressed in decibels (db.) above the absolute threshold. The decibel scale is less widely used in reference to the intensity of sensations of brightness. The luminance of the flickering spot in these experiments was 98 mL. or 85 db. above the absolute threshold of vision. This corresponds to a moderately bright light in about the same sense that the 100 db. noise in the flutter experiment is a moderately loud noise.

[2] Shower and Biddulph (1931).

[3] Miller and Taylor (1948).

[4] Mowbray and Gebhard (1954).

[5] The present experiment.

Pitch is a fused, qualitatively unique experience and may not belong in the table at all. Flutter and flicker, however, are more alike. Both are experienced as intermittance at low rates of interruption, but tend to change qualitatively as the rate is increased. Flutter, for example, takes on a tonal character at about 40 cps., becomes noise differing in quality from steady noise at 250 cps. and finally is indistinguishable from steady noise at about 2,000 cps. Flicker, too, passes through various phases from slowly alternating light and dark intervals at low frequencies to a sensation of fused brightness at about 60 cps. (Bartley, 1941). To these qualitative difficulties in comparison are added two others. First, there are only two frequencies in available measurements where flutter and flicker overlap; and second, Miller and Taylor used a method of constant stimuli in collecting their data, whereas

we used a method of average error. Whatever these differences may entail, it is noteworthy that the DLs for flicker are respectably small and that they hold their own when compared to their auditory counterparts.

Work on this problem is being continued by investigating the effect that various visual parameters have on the DL. Currently under examination are the roles of spot intensity and the light-dark ratio.

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A NEGATIVE TEST OF THE DRIVE-REDUCTION HYPOTHESIS

BY

RICHARD A. LITTMAN and EDWARD A. WADE*

From the University of Oregon and the University of Wisconsin

A major deduction of the drive-reduction hypothesis of reinforcement was tested. A light which follows the termination of a shock coincides with a condition of drive or need reduction. If drive reduction is a sufficient condition for reinforcement, then the light should become a secondary reinforcing agent by virtue of its association with the drive-reduction event. If it becomes a secondary reinforcing agent, it should be able to reinforce an instrumental act.

This sequence of deductions was tested by placing rats in a stock, giving them a brief shock upon the termination of which a light was flashed for one second. After sixty such pairings, subjects were then placed in a Skinner box under operant test conditions; when they pressed the bar a light flashed on.

Animals subjected to this training programme were compared for rate of bar pressing with animals subjected to a variety of control conditions. No significant differences were found among the groups, and it was concluded that in so far as the deductions from drive-reduction theory were correct, the theory was not substantiated. The paper concludes with a discussion of various problems which might limit the interpretation of the experimental results.

In order to explain a great many instances of learning where there is no evident reduction in a primary need, reinforcement theorists have found it necessary to postulate a process of secondary reinforcement. Unfortunately, there has been little attention devoted to the conditions under which secondary reinforcing agents are established. For the most part, research has been concerned with demonstrating that such agents exist.

In this respect, Hull's system is unusual, since he specifies the conditions for establishing secondary reinforcing agents. He writes "... a receptor impulse will acquire the power of acting as a reinforcing agent if it occurs consistently and repeatedly within 20 seconds or so of a functionally potent reinforcing state of affairs, regardless of whether the latter is primary or secondary" (1943, p. 95). A reinforcing state of affairs is "... the diminution in the receptor discharge characteristic of a need or ... a stimulus situation which has been closely and consistently associated with such a need diminution" (1943, p. 95); the latter portion of the quotation, of course, describes a secondary reinforcing agent. Hull further states that "... a stimulus alone is ineffective as a secondary reinforcing agent" (1943, p. 98) and that "Stimuli which acquire secondary reinforcing power seem always to acquire at the same time a conditioned tendency to evoke an associated reaction" (1943, pp. 97–8). *These last two statements, it should be noted, are in the nature of empirical observations; they are not deducible in the theory.* However, the reader's attention is drawn to them because they would appear to undercut the experiment described below; in so far as they do not play the role of formal elements in the theory—indeed, since they are in contradiction with the formal theory—it must be assumed that they cannot be used to justify or attack the theory itself.

* The data reported here were collected by Mr. Wade for his M.S. degree at the University of Oregon (1951).

It seems clear from the above that in order to establish a secondary reinforcing agent it is necessary only to present some stimulus condition (thing or event) in close temporal relation to the reduction of a drive. The subsidence of hunger, thirst or sexual tension, the relief from shock or escape from a frightening situation are illustrations of events which may be treated as primary reinforcements. The present investigation is an attempt to test this implication of the drive-reduction hypothesis of reinforcement.

In his later publications Hull (1951, 1952) introduced what would appear to be a radical change in his definition of a reinforcing state of affairs. While earlier he spoke of a reduction in a need condition, he shifted later to the concept of drive-stimulus diminution. The present paper is couched for the most part in need-reduction language; however, unless one accepts an extremely radical version of the drive stimulus, viz., that it refers only to a non-organismic stimulus variable, the picture remains quite the same. At any rate, the investigation to be described rests on the assumption that the association of a stimulus condition with the reduction in pain following shock termination should be *sufficient* to establish a secondary reward agent. Therefore, if this stimulus condition subsequently occurs within 20 seconds or so of a receptor-effector connection, it should prove to be a reinforcing agent by increasing the frequency of the response.

The following steps were taken in line with this argument: albino rats were enclosed in a restricting tube and given a fairly strong shock for one-half second through electrodes attached to the tail. One-tenth of a second after the shock was terminated they were stimulated for one second by a light which reached them through a milk screen covering the end of the tube in which they were contained. This was done for sixty trials in an experimental group. Subsequently, the animals were placed in a Skinner box under operant level test conditions. Each time the bar was pressed, the same light which flashed on while they were in the tube flashed on and stayed on for one second. While the selection of the particular time intervals used was based on sheer hunch, they are well within the ranges specified by Hull. Also, we have accepted an assumption of Hull's that the removal of shock is reinforcing since it results in a reduction of the drive instituted by the shock.

There is one other feature of the investigation for which Hull provides a basis. We have taken a stimulus appearing in one context (the tube) and used it to reinforce behaviour in an entirely different situation, the Skinner box. In this respect, our investigation is more "radical" than Coppock's (1951) or Schoenfeld's *et al.* (1950), which are the only other studies in the literature that have considered the same problem. Speaking of Frolov's experiment, Hull asks, "Is this typical of secondary reinforcement, i.e., is secondary reinforcement confined to the transfer of the same reaction from one stimulus to another, or may any receptor conjunction be connected by secondary reinforcement?" (1943, p. 86). On the basis of studies by Skinner and Bugelski he rejects the necessity for such a limitation on the process of secondary reinforcement. Further, he states that it is not necessary that a secondary reinforcing agent be related to the response involved in its conditioning in such a manner that it evokes that response. In other words, a secondary reward agent is, in Meehl's terminology, a "trans-situational reinforcer" (1950), and there is evidently nothing in the Hullian system which would argue against the particular test situation we have selected.

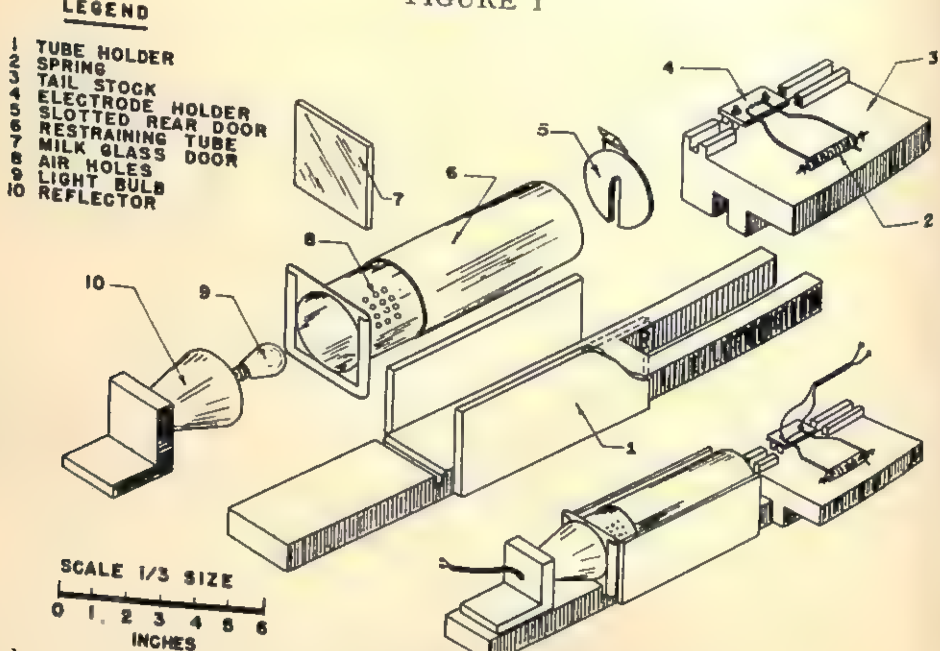
One thing concerning the procedures selected should be pointed out. We have attempted to utilize *only* the formally stated postulates and corollaries. There are all sorts of auxiliary considerations which, in one place or another, are brought in by Hull. It is obvious, however, that though it may involve a certain measure

of unfairness to a theory to be treated in such an exacting manner, it is the only way that will yield a sound evaluation. Indeed, to do anything else is really to do a disservice to a theorist's intentions, at the very least.

Method

Subjects. The subjects were 30 female albino rats of the Holtzman-Sprague-Dawley strain, 100–105 days old at beginning of training. Upon receipt in the laboratory they were placed in individual living cages and taken out every day for handling. After their ears were punched for identification several days later, the animals were arbitrarily assigned to specific training groups. Following this assignment they were put on a regular schedule of 15 grams of lab chow daily with water present in the living cages at all times.

FIGURE 1



Schematic drawing of restraining apparatus. The assembled device is shown in the lower right; above it is an exploded view

Apparatus. During the training phase the animals were held within a *restraining tube* (Figure 1). This consisted of a collapsible tube two inches in diameter which was adjustable to the size of each animal. It was similar to one described by Herbert (1946), the only modification being the substitution of a *milk-glass* door for an opaque door at the front end. A *slot* in the rear permitted the tail of the animal to protrude from the tube. The tube was placed in a *holder* which could be set down within the Skinner box. Behind the slotted portion of the tube a *tail-stock* was mounted on the holder. This consisted of a pair of electrodes which were attached to a small mouse-trap spring to keep the animal's tail from jerking out of the stock. This seemed to work well and avoided the problems associated with clipping electrodes to the tail. The electrodes were made of brass with a coating of felt that was saturated with a salt solution to insure conduction.

Mounted on the holder in front of the milk-glass door of the tube was a six-watt, clear, candelabra-size bulb encased in a cone-shaped reflector. The glass effectively diffused the light so that the animals were stimulated by a two-inch diameter circle of light.

Shock was administered from a toy transformer through an inductorium. Inclusion of a voltmeter and milliameter in the circuit permitted periodic checks on, and adjustment of, the amount of shock delivered to the tail.

The *Skinner box* was sound- and air-conditioned. The inside dimensions were 11 × 11 × 14 inches long. A removable T-bar made of one-eighth inch brass rod, projected through a slot for a distance of 1 inch at a height of 2 inches above the floor. The bar required 12 grams pressure to activate the micro-switch attached to it. Immediately above the T-bar was a 2-inch diameter milk-glass screen, similar to that mounted at the front of the restraining tube used during training; its centre was 2 inches above the bar. The same light source used in the training sessions was placed behind this screen and could be activated by depressing the T-bar. If the bar was held down it "timed out" automatically after one second. Rapid pressing of the bar, however, could make the light go on and off.

The interior of the box was painted flat-black and illuminated during all phases of the experiment by a small blue Christmas tree bulb at the rear of the box. This was turned on during the training phase after the animal, already inside its tube, was put into the Skinner box.

Stimulus control was effected by a three-component electronic timer, which controlled shock duration, inter-stimulus interval, and the duration of the light stimulus.

Design and Procedure

There were three phases: pre-training, training, and test. These are summarised in Table I.

TABLE I

Experimental Procedure

Stage				
I. Adaptation Period (3 one-hour sessions)	Adjustment to laboratory routine, adaptation to E and to apparatus			
II. Training Session (6 sessions, 10 trials per daily one-hour session)	Group I (N = 7) Group II (N = 3) Shock-light (see text for durations)	Group III (N = 8) Shock stimulus only	Group IV (N = 7) Light stimulus only	Group V (N = 5) No stimuli at all
III. Test Situation (4 sessions, one-half hour each)	Group II Skinner box (no light on press)	Groups I, III, IV, V Skinner box situation one-second light follows each bar press		

(1) *Pre-training.*

Each animal had three sessions during which he was accustomed to the investigator and the restraining tube. Each session consisted of 15 minutes in an isolation box, 5 minutes on the experimenter's lap (and any other portion of his anatomy the animal could reach), 20 minutes in the tube, 5 minutes on the experimenter's lap again, and then 15 minutes in the isolation box. Fifteen minutes after their removal to the home cage from the isolation box they were fed their daily ration of 15 grams. Adjustment to the restraining tube was "non-directive." It rested in the experimenter's lap and the animals were permitted to explore it in any way they wished. They quickly used it as a runway from one side of the experimenter's lap to the other. The tail stock was introduced during the third session.

(2) *Training.*

Each animal received 60 trials at the rate of 10 a day. These trials took place in the restraining tube placed within the Skinner box. Each session occurred 23 hours after the last feeding, and was preceded and followed by a 15-minute session in an isolation box. The subjects were fed 15 minutes after returning to the home cage.

In transporting the animal to the Skinner box from the isolation chamber, extraneous visual stimulation was controlled by placing the animal in the restraining tube and

covering the glass door until the tube was safely placed within the box. Return to isolation was in the same manner. All animals received a 5-minute "rest" in the stock prior to the first trial and following the last one of each daily session.

Training occurred in six blocks of 10 trials each. These blocks contained various arrangements of randomized inter-trial intervals ranging from 1 to 6 minutes in length. Each animal received every block of trials in a counter-balanced arrangement.

All three phases of the experiment involved considerable distribution of practice; each session occurred four days after the previous one. By taking one animal from each group in regular order, it was possible to have at least one animal of each group run at the same daily time. Groups IV and V were separated from I, II and III by two months.

The training procedures for each of the five groups is given below.

I. *Experimental group: shock-light* (N = 7). A non-avoidance procedure was employed with the seven subjects in this group. For each of the 60 trials, a 70–90 volt A.C. shock of about 0·2 milliamperes was administered for one-half second. One-tenth of a second after its termination there occurred a light of one second duration. The same light stimulus occurred during training when the T-bar was depressed.

II. *No-test-light control group* (N = 3). These animals received precisely the same training as the previous group but there was no light during the test situation.

III. *Shock control group* (N = 8). These animals received 60 trials of shock, no light following. The light stimulus was present in the subsequent test situation.

IV. *Light control group* (N = 7). This group received light alone during training. The light was also present during the test situation.

V. *Light-shock control group* (N = 5). The animals were placed in the restraining tube but no shock or light was administered. Total elapsed time was 40 minutes per session, which equalled the average time spent in the tube by the other four groups. In the test situation this group served as a base-operant reference point when, for them, light was present for each bar depression.

(3) *Test.*

Four test sessions of one-half hour each were given all animals. Again all sessions were preceded and followed by a 15 minutes' stay in the isolation chamber. Since the animals were not fed until after each session, they were operating under a fairly strong hunger drive. Informal observation indicates that they tended to consume most of their daily ration within a brief period after it was given them. Presumably there was also some exploratory drive present.

Each animal was placed in the Skinner box facing the front end. The restraining device was, of course, absent and facing the animal was the T-bar. In addition to the automatic recording of bar presses, the minute-by-minute activity of the animal was recorded by the experimenter. It will be recalled that immediately above the bar was a circle of glass. This was illuminated for each response in the case of the following groups: shock-light (I), shock-control (III), light-control (IV), and light-shock control (V).

The animals were placed in a restricted environment for the first session. A small box, 6 × 6 × 9 inches long with open front, rear door and glass ceiling, was placed with the Skinner box and directly in front of the bar. This was done to centre activity in the region of the bar and to simulate the restraint of the tube during training. Thereafter, they were simply placed within the box, head facing the bar, and permitted to approach the bar at their own pace.

Results

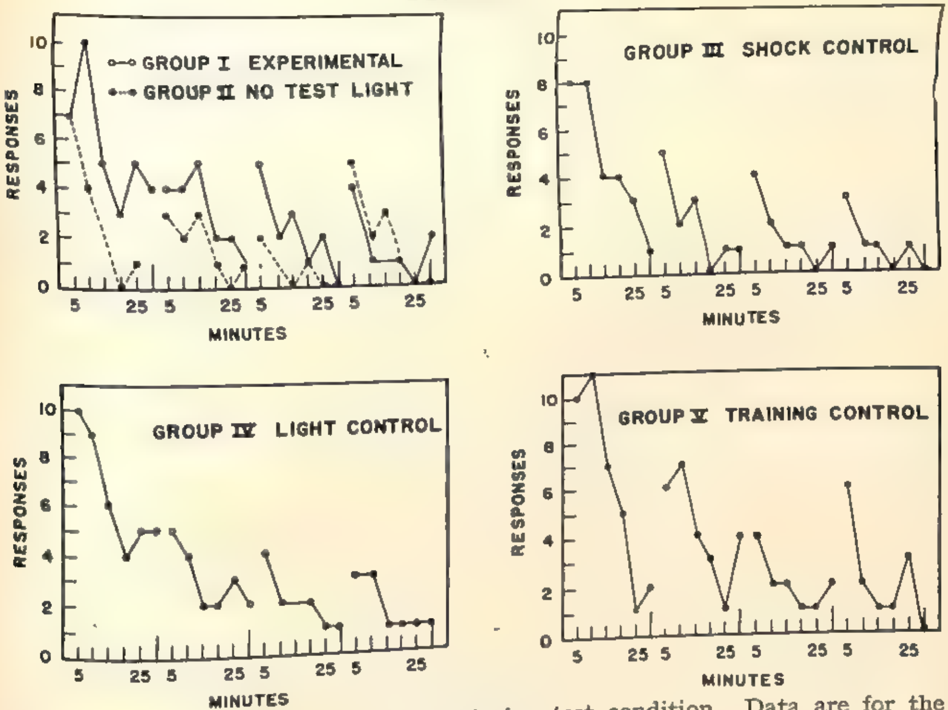
The significant relations to be examined in this study are those among the five groups, especially the contrast, if any, between group I and the others.

In Figure 2 will be seen the response curves for the five groups for each of the four test sessions. Two things are clear merely from inspecting these curves: (*a*) all five groups show the extinction pattern typical of operant tests; (*b*) the experimental group (I) does not appear to run a course that differs from that of the other groups.

Both of these impressions are confirmed by the statistical analysis. Alexander's test for trend (1946) was used to test for the significance of difference among the

curves. For economy's sake, we have analysed the *totals per session* rather than the *totals per five minutes* on which the graphs are based; there seems little reason,

FIGURE 2



Mean responses per 5-minute periods during test condition. Data are for the four test sessions

however, to believe that the results would have been different. The results of the analysis may be seen in Table II.

TABLE II

ANALYSIS OF VARIANCE FOR TREND IN THE PERFORMANCE SCORES OF FIVE GROUPS OF RATS TESTED IN A SKINNER BOX SITUATION FOLLOWING EXPOSURE TO DIFFERENTIAL PRE-TEST TRAINING

The analysis is based upon a square-root transformation of raw data secured as total bar responses during each of four test sessions

Source	*Sum squares*	*d.f.*	*Mn. sq.*	*F*[1]	*F*[2]
a. Overall slope	102·33	1	102·33		
b. Overall deviations from linearity	16·65	2	8·32	13·64*	
c. Between group means ..	17·98	4	4·49	7·36*	1·43[2]
d. Between group slopes ..	6·14	4	1·53	2·51	
e. Group deviations from estimation	6·68	8	0·83	1·36	
f. Between individual means	78·26	25	3·13	5·13*	
g. Between individual slopes	15·57	25	0·62	1·01	
h. Individual deviation from estimation	30·50	50	0·61		
Total	274·11	119			

[1] The fundamental error term is *h*.
[2] The error term is *f*.
* Significant at the 0·001 level.

The assumptions for using the analysis of variance have been satisfied. In order to achieve normality the original response data were subjected to a square-root transformation. Bartlett's test for homogeneity was applied to the various groups and none of the F's proved significant at the 5 per cent. level.

Let us consider first whether there are any significant changes in response rate for all animals over the four sessions. In terms of Alexander's analysis, we are interested in the overall deviations from linearity. These are highly significant when tested against the individual deviations from estimation. There is, therefore, a marked decline in response rate and the decline is curvilinear. It is worth noting that the actual rates themselves are exceedingly close to those reported in the literature for base operant tests (Schoenfeld, 1950).

Now, how about the differences among the five groups? The tests which are relevant here, *between-groups-slopes* and *between-groups-means*, are both significant. As so often occurs in dealing with the analysis of variance, a problem arises concerning the appropriate error term to use in testing the means. When they were tested against the fundamental error term, the individual deviation from estimation, F was 7·36, which is significant beyond the 0·001 level. However, the between-individual-means was also significant when tested against the fundamental error term. Consequently, we have used it as the more appropriate error term to evaluate whether there was a significant difference among the means. As can be seen in Table II, with this mean square as error, F is no longer significant; the means of the five groups do not differ significantly from one another.

Let us summarize these results. All groups follow the same decreasing curvilinear trend of responding over the four sessions. However, there are no differences among the five groups either with respect to slopes or means.

Discussion

The results are quite unambiguous. Indeed, it is surprising how similar the response curves of the five groups are. The differential training and test conditions appears to have had no effect upon the behaviour of the animals, all of whom behaved exactly as untreated base operants do.

Nevertheless, there are for this study, as for all studies, limiting conditions which must be considered. There are five which merit discussion.

(1) *The shock may not have been strong enough.* In one of his earlier studies, Miller (1948) found that a low shock level did not produce fear-reinforced learning. While the difficulty in evaluating the strength of shock-input at the contact point with the electrodes is well known, Miller's procedures suggest that this upper-level shock was not far from tetanic. The shock level in the present study does not appear to be too different from the low-level shock which proved inadequate in his study. Hence, it is possible that had we used a higher shock level positive results might have been obtained.

While we admit the cogency of this point, there are several items to be considered. Essentially, it is an argument that drive strength must be beyond a certain level before it can be effectively reduced so as to produce some learning. The reason for this is as follows. Drive is an inferred state; it is inferred from a variety of schedule and training procedures, and response indices. If it is said that the strength of a shock is too low to be efficacious, one is, in effect, saying that the degree of drive reduction is below a given value. But what is the evidence in the first place that drive exists? Generally some particular class of overt responses; in the case of shock these are such things as jumping, squealing, jerking, etc. Now, these are precisely the kinds of behaviours exhibited by our animals in the tube when shocked *throughout*

the entire schedule of 60 *trials*. If, then, drive reduction is contingent upon the cessation of shock (which produces the drive), then since our animals satisfy the behavioural criteria for drive induction they must also satisfy the criteria of drive reduction when the shock is terminated. Usually, this is taken to be an increasing quiescence in the case of shock; this our animals very clearly exhibited.

(2) *The shock may not have lasted long enough*. The shock in our study lasted about one-half second. While it did set up behaviours which satisfied the test of a drive state, it still may not have lasted long enough. While this criticism does not stem from the present formulation of the drive reduction hypothesis, it is worth considering. In the Wolf (1936) and Cowles (1937) experiments with chimpanzees, and Miller's studies of fear learning (1951), the drive state is presumably continuous-hunger or response-terminated shock. These seem to produce effective learning. In our study, the procedure was one of classical conditioning in which the shock was terminated by the experimenter prior to the light's appearance.

This suggests that the duration of the shock may have two possible effects. (*a*) It may have to be of a certain duration in order to build up the drive to a particular threshold value. If this is the case, then shock duration becomes a special case of shock strength. (*b*) But the shock may have to be of a certain duration in order to permit the animal to make certain kinds of discriminations. If so, then drive reduction may be effective only if certain perceptual responses also occur. This, of course, is the point of view which Tolman and other cognitive theorists insist upon. In any case, we have no basis for knowing just what shock duration is critical at the present time. That duration is critical for some types of behaviour, however, cannot be doubted as the work of Solomon and his co-workers at Harvard clearly shows (1953).

(3) *The light appeared at the wrong time*. Hull stated that if the "receptor impulse . . . occurs . . . within 20 seconds" of drive reduction it will be sufficient for establishing a secondary agent. This estimate may have been incorrect, but we cannot be sure since it covers such a broad interval. In any case, we know from the present study that the critical interval is not one-tenth of a second after shock-termination. While this eliminates only one of many possible intervals, it is again necessary to know more exactly the proper limits and the sequence of events within which a secondary agent can be expected to develop.

We shall not discuss in any detail the matter of the order of appearance of CS, US or CR. There exists a tremendous literature which is still to be clarified by further investigations. It does appear, though, that Hull was attempting to gather many eggs into one basket when he assumed that the order and temporal relations which seemed to operate in the conditioning of receptor-effector connections also applied to the conditioning of secondary reward agents. There is, of course, no reason why this must be the case. It is our suggestion that the postulated drive reduction condition as a common factor is responsible for the extreme generality of Hull's formulation. If one does not proceed from this point of view concerning the origin of secondary reward agents, it is quickly apparent that a variety of possibilities of the sort reviewed by, say, Keller and Schoenfeld (1950) exist.

(4) *Number and distribution of trials*. These are important considerations. It will be recalled that 60 pairings of light-shock were used before the subjects were tested for conditioning in the Skinner box. What would have happened had fewer or more pairings been used? We cannot say, of course, and obviously a parametric study of just this sort is indicated. Even so, it does not seem likely that the strength of the secondary agent would be diminished as the number of pairings increased. While the increments would decrease, according to Hull, the absolute value would

continue to increase from trial to trial. Hence, unless some additional factor supervenes during the course of conditioning, there seems no reason to expect the to-be-conditioned secondary agent to become less likely to act as a reinforcer as training continues.

The matter of the degree of distribution of practice cannot be as readily evaluated. It would appear, in classical conditioning at any rate, that ten trials a day is not an optimal rate (Schlosberg, 1934). Even so, 60 trials, when applied in blocks of ten trials each, should produce some effect if there is any to be achieved. Perhaps it is the separation of each session by such a lengthy period as four days rather than the number of trials per session which is involved. To neither of these questions can we give any definite answer except to point to the general superiority of distributed over massed practice.

(5) *Logical considerations.* There is, finally, the possibility that Hull's theory simply does not predict what we say it does. While the introduction does contain the essential information to put such an objection to rest the matter can be put differently.

There are three lines of doubt concerning the relevancy of our design to Hull's theory: (*a*) one raises the question of the relation of acquisition of secondary reinforcing powers to the conditioning of a stimulus-response sequence; (*b*) the second deals with the appropriateness of the test-condition drive to reinforcement; (*c*) the third has to do with the matter of the secondary drive properties of the light signal.

(*a*) Is it necessary that there be a receptor-effector relation conditioned at the time a secondary reinforcing agent is established? Because of the informality of his writing, the close conjunction of statements concerning primary and secondary reinforcement and the final "reduction" of the secondary drive and reward postulates to corrolaries in his more recent writings, it would appear that this is what Hull says. However, such is not the case; it stems from a kind of "guilt by association" process induced in the reader.

It is readily seen that the association between these concepts stems from the role of drive reduction (need diminution earlier, drive stimulus subsidence later) as a sufficient condition for both outcomes, viz., strengthening an S–R relation in the one case and establishing a secondary agent in the other. Beyond the newly-acquired power of a secondary agent to reinforce S–R connections, there is no connection between the two events.

It is instructive to consider why this is so. The notion of primary reinforcement is introduced in connection with the role of reinforcement in strengthening an S–R bond. However, what constitutes a primary reinforcing state of affairs is independently defined. Hence, it can occur even when there is no S–R relation to be strengthened (though other portions of Hull's system would make this quite unlikely actually). Therefore, a reinforcing state of affairs can occur in the presence of a stimulus object or event under circumstances where no S–R relation occurs and presumably generate secondary reinforcing powers in that agent or event.

(*b*) What drive was present during testing which could be reduced by the light? Under conditions of secondary reinforcement there is no need for such a drive. Indeed, it was to provide for just such a circumstance, where learning occurs in the absence of any apparent drive reduction, that the secondary reinforcement principle was devised. Perhaps it was just this which led Hull to re-word his statements concerning what was the reinforcing condition in his later writings.

Since it is still of some concern as to what the drive status might be presumed to be during the test situation let us point that out here. During both training

and test conditions the animals were operating under a 22-hour hunger drive, so some motivational conditions can be assumed to have been at work.

For those who read Hull differently from the present authors and who feel (1) that some relation of appropriateness of drive to the two conditions should be raised, and (2) that some drive stimulus reduction is implied by Hull even for secondary reinforcement, the following observations may be helpful. During training, a hunger drive and escape-from-shock-drive were jointly present. The diminution in pain from shock followed some stimulus components of the hunger drive; hence, by primary reinforcement, these components became conditioned to the escape-from-shock movements. But these same stimuli, by contiguity with the diminution in drive state, will also become drive stimuli and hence provide the animal during the test situation with at least some component equivalent to the drive state upon which the secondary reinforcing agent was based.

(*c*) Shouldn't the secondary drive factor have played some role in the behaviour of the animals during the test situation? We have already indicated in connection with (*b*) above how it could have operated. One could, by further analysis, explore the possibilities that it might have been expected to raise or depress the response rate during the test circumstance. But this sort of tortuous reasoning is scarcely justified since there seems to be no more basis for believing that a secondary motive was formed than that a secondary reinforcer was developed.

Now what conclusions do our results suggest? It appears obvious that the drive-reduction hypothesis of reinforcement has yielded an unverified prediction. There is no reason, however, why the hypothesis cannot be formulated so as to avoid the particular problem we have posed and tested. If the various postulates bearing on the need-reduction hypothesis of reinforcement are reworded and made more exact with respect to such variables as order of events, temporal relations, shock strength, and so on, it will surely be more rigorous. It should, by the same token, become much more useful.

On the other hand, it should be noted that most of our criticisms have stemmed from the general area of reinforcement theory. Many of the *facts* of primary and secondary reinforcement are certainly not at issue. It is a particular hypothesis about how reinforcement operates that is. To that extent, one can still use the notion of primary and secondary reinforcement (or goals and sub goals, in Tolman's language) with much of the explanatory richness and power that they provide.

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EFFECTS OF NITROUS OXIDE ON REACTIONS TO "STRESS"

BY

ROGER W. RUSSELL and HANNAH STEINBERG

From the Departments of Psychology and Pharmacology, University College, London

It has been demonstrated that exposure to the stress of a situation involving conflict or frustration may interfere significantly with subsequent learning. Behaviour under stress is characterized by experiences of "anxiety" and by widespread physiological changes. Inhalation of nitrous oxide has the general effect of reducing the extent of these symptoms.

The present investigation using insoluble and soluble problems has demonstrated that both nitrous oxide and exposure to stress impair learning; but that, when subjects are exposed to stress while under the influence of the drug, the effects of the stress on subsequent learning are abolished. It is suggested that these empirical results may be accounted for either in terms of "anxiety-reduction" or in terms of transfer effects.

Introduction

Since Pavlov (1941) and his colleagues first described the symptoms of what they called "experimental neurosis" in dogs and specified certain conditions under which these symptoms could be produced, there has developed an increasing interest in experimental approaches to psychopathology. Much has been learned of the critical conditions affecting the organism and its environment which are associated with disorganization of responses or the appearance of aberrant behaviour patterns. These critical conditions have been referred to as "stress" and behaviour changes in organisms exposed to them, as "reactions to stress" (Russell, 1953).

Among the symptoms shown by organisms exposed to stress are experiences which human subjects report as "feelings of fear" and "apprehension." These experiences are often accompanied by widespread physiological changes, including changes in heart rate, blood pressure, respiration and increased tension in the striped musculature. Both clinical and experimental studies in psychopathology have referred to these particular symptoms under the concept of "anxiety." "Anxiety" has been defined as ". . . a learned response, occurring to 'signals' (conditioned stimuli) that are premonitory of (i.e., have in the past been followed by) situations of injury or pain (unconditioned stimuli) . . ." (Mowrer, 1950). Recent experiments with the anaesthetic gas, nitrous oxide (Steinberg, 1953) have indicated that, while efficiency in the performance of a wide variety of tasks deteriorated under its influence, the subject's general behaviour tended to be characterized by euphoria, i.e., "elation," "optimism" and a general reduction of anxiety and tension.

The present experiment is the first of a series planned to investigate the effects of this apparent anxiety-reduction on reactions to stress. Subjects have been exposed under various conditions to the stress of an insoluble problem situation and the immediate effects of this exposure on their behaviour while attacking a soluble problem have been observed. This technique has been made prominent by Maier (1949) and his students in research involving infrahuman animals and has been applied to human subjects by Marquart (1948), Jones (1952) and others. All have reported effects which they claim are attributable to the stresses imposed on their subjects. In the present experiment three main hypotheses have been tested: (i) performance of a soluble task is less efficient, in terms of trials and errors to reach

a criterion of learning, when subjects are under the influence of the drug; (ii) performance of a soluble task is less efficient when subjects have previously been exposed to an insoluble task; (iii) if subjects are under the influence of the drug while exposed to an insoluble task, the effects of such exposure on the subsequent performance of a soluble task will be reduced or abolished.

Experimental Design and Procedure

I. Experimental design.

The design of the experiment is shown in Table I. It required five groups of subjects, the groups receiving different treatments in terms of presence or absence of the drug and/or exposure to stress. By making appropriate comparisons among the various groups, it was possible to test each of the hypotheses stated above.

TABLE I

Experimental Design

Group	*Phases* 1 *Addition*	2 *Insoluble task*	3 *Soluble task*
1	No drug	—*	No drug
2	Drug	—*	Drug
3	No drug	No drug	No drug
4	Drug	Drug	Drug
5	Drug	Drug	No drug

* In these cases the subjects proceeded directly from the addition to the soluble task.

All subjects were kept occupied with a series of addition problems for the first five minutes of the experiment before starting work on the insoluble or soluble tasks. During this period the drug was taking effect in subjects of groups 2, 4 and 5. Decrements in performance of the addition problems served to indicate that the subjects were indeed being affected. For the treatment of group 5 it was important that the effects of the drug should subside rapidly after administration ceased. That this would be the case with nitrous oxide was to be expected on the basis of pharmacological evidence and was substantiated by the results of the present experiment.

II. Subjects.

Five groups of eight subjects were treated as shown in the experimental design. All subjects were male student volunteers, aged between 18 and 24. They were assigned at random to the five experimental groups.

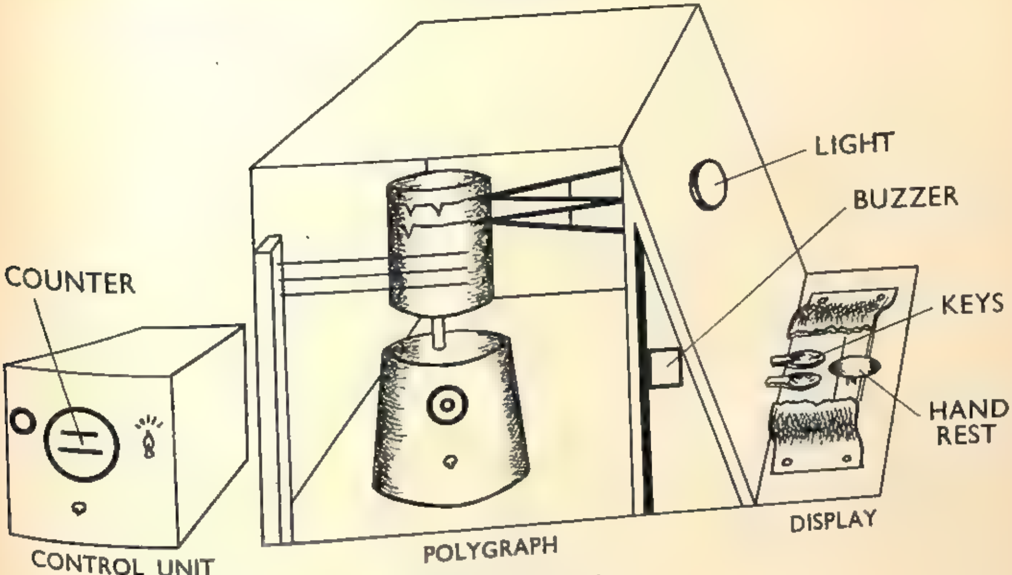
III. Apparatus.

1. *Administration of drug.* All subjects breathed through a face mask connected by rubber tubing to gas flow meters. Those receiving the drug were given 30 per cent. nitrous oxide in oxygen, scented with lavender; the others, similarly scented air. The apparatus has been described in detail elsewhere (Steinberg, 1954).

2. *Addition problems.* The addition problems consisted of a set of single-digit numbers placed at equal distance along a row. All problems were soluble and the digits were selected from a table of random numbers. There were ten different series of such problems.

3. *Insoluble and soluble tasks.* A diagram of the apparatus employed for presenting the insoluble and soluble tasks is shown in Figure 1. These tasks consisted

FIGURE 1



Learning apparatus

of temporal maze problems involving a number of choice points, at each of which a subject could make one of two alternative responses. The insoluble maze was one in which the temporal order was so complex that solution within the time limit allowed was impossible. The temporal order of the soluble maze was much simpler and was learned by all control subjects receiving no drug or stress. The mazes used were selected on the basis of a series of preliminary studies. The apparatus consisted of two spring-loaded keys operated by the subjects; a black screen on which were mounted two light signals, one red and one white; and an auditory signal, a buzzer. The signals were controlled by mechanically operated switches which could be set in predetermined sequences by the experimenters. Correct responses were indicated by the appearance of the white light and incorrect responses by the sound of the buzzer. The red light went on at regular intervals of five seconds, indicating to the subject that he must make his next response. The appearance of the signals and the subjects' responses were recorded by ink traces on a moving paper polygraph.

IV. *Procedure.*

On entering the experimental room the subjects were asked to fill in a general information sheet and were then given instructions regarding the addition and maze tasks they were to perform. These instructions were the same for all five groups.

The subjects were instructed to start each addition problem from the left of the row of numbers and to add the numbers until they totalled 15. At the end of each 30-second period during the five minutes devoted to addition, he would be told to begin a new series of problems. Performance during each of these 30-second periods could be measured in terms of correct responses and errors, and trends in performance during the five minutes of adding could be determined.

The subjects were then shown how to operate the maze apparatus. Responses were to be made by depressing either the right or the left key with the appropriate first or third finger of the preferred hand. Enough pressure had to be exerted to reach the contact point attached to micro-switches associated with each key. The subjects were told that, during the experimental period, the red signal light would appear at 5-second intervals. This would indicate that the next choice point in the maze was being presented and they were to respond by pressing once one or other of the two keys. The task was to learn, as quickly as possible, a sequence of left and right pressings which continuously repeated itself. Correct responses would be indicated by the appearance of the white light and incorrect responses by the sound of the buzzer. The lengths of the sequences were never disclosed. These instructions ended with six practice trials.

The actual experiment then began. The mask was fitted to the subject and he started inhaling the gas—drug or air—required for his particular group. All subjects spent the first five minutes working on the addition problems. Subjects in groups 1 and 2 were then put to work on the soluble maze, which consisted of a sequence of five pressings, viz. left-left-right-left-right. The learning criterion adopted was 20 consecutive responses without error, and as many trials were allowed as were needed for solution, up to a maximum of 300. This relatively simple criterion was decided upon after preliminary studies in which it was found that more severe criteria did not affect the number of trials to learn. Each appearance of the red signal to respond was counted as a "trial" even if, as happened occasionally, the subject failed to make a response. Groups 3, 4 and 5 were first presented with the insoluble maze. After 50 trials these subjects were stopped and told that they would now be given a new and different sequence to learn; they were then put to work on the soluble maze.

After the experiment was completed each subject was questioned regarding his experiences during the experimental period. At no time was the subject informed of the purposes of the research.

Results

I. *Effects of nitrous oxide on addition.*

Table II shows the effects of the drug on performance of the addition problems. It will be noted that the drug groups—2, 4 and 5—solved significantly fewer problems and made significantly more errors than did groups 1 and 3, which were inhaling air.

TABLE II

Effects of Nitrous Oxide on Addition*

Groups	*Treatment*	*Median correct*	*P*	*Median errors*	*P*
1, 3	No drug	106·0	0·02	1·5	0·005
2, 4, 5	Drug	79·5		4·0	

* One-tail tests were used since the prediction was that performances would be poorer with the drug.

Significance is defined in this instance and throughout the paper in terms of the 5 per cent. level of confidence and was tested by the Mood (1950) and Mann and Whitney (1947) tests. These results were expected on the basis of previous research (Steinberg, 1953) and indicate that the drug had taken effect before the mazes were presented.

II. Effects of nitrous oxide on learning.

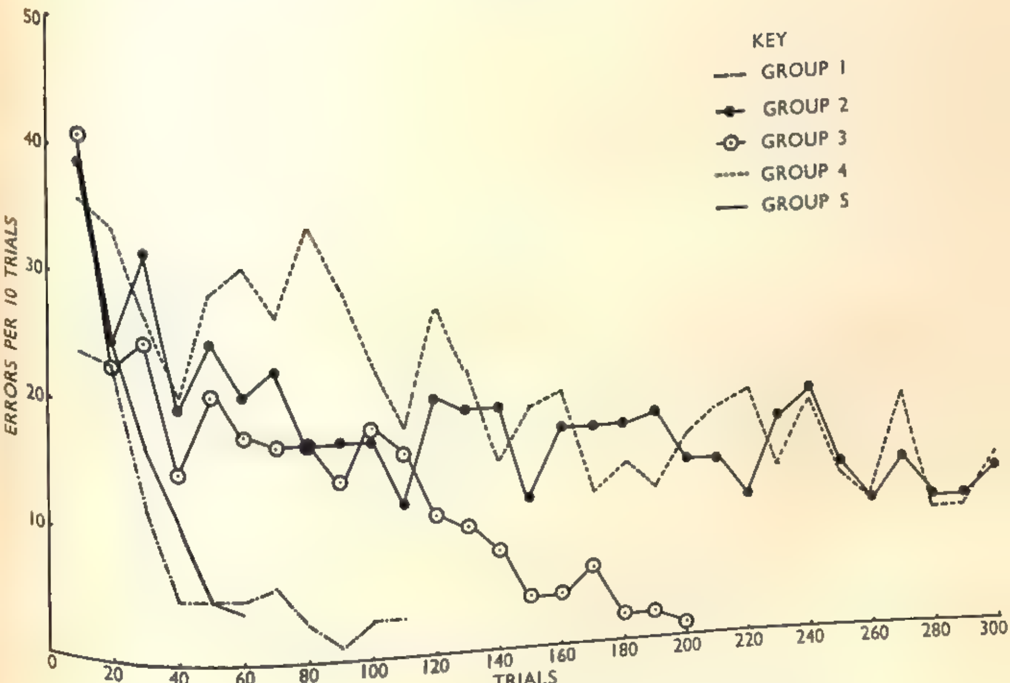
Table III summarizes the performance of the various groups in learning the soluble maze. The data presented are in terms of the number of trials taken to reach the learning criterion. These data are essentially similar to those expressed in terms of errors to reach the criterion, since trials and errors were highly correlated ($r = 0{\cdot}96$). Figure 2 shows learning curves for the five groups plotted in terms of total errors per ten trials for each group.

TABLE III

TRIALS TO LEARN THE SOLUBLE MAZE

	Groups				
	1	2	3	4	5
Median	36·5	276·5	104·0	214·0	31·0
Range	6–110	27–300	10–200	71–300	19–59

FIGURE 2



Learning curves (errors) for performance of soluble maze

The effects of nitrous oxide on learning of the soluble maze when no stress was introduced can be determined by comparing the data for groups 1 and 2, the former having inhaled air and the latter, the drug. The drug group was much slower in reaching the criterion than the air group, this difference in performance being statistically significant ($P = 0{\cdot}007$). The learning curves for these two groups also differ markedly, that for the drug group showing a slower elimination of errors and a higher terminal error level. This higher terminal level is due to the fact that

four of the eight subjects who inhaled the drug failed to solve the maze in the 300 trials allowed.

III. Effects of exposure to stress on subsequent learning.

Comparisons of groups 1 and 3 in Table III and Figure 2 show the effects of exposure to stress on subsequent learning of the soluble maze. The stressed group took significantly more trials to learn ($P = 0{\cdot}03$) and eliminated errors at a slower rate, although all subjects eventually solved the maze. It is interesting to note that the performance of the stressed group lies between the performances of the air group and the drug group.

IV. Effects of simultaneous exposure to stress and nitrous oxide on subsequent learning.

Group 5 was exposed simultaneously to stress and to nitrous oxide, and it was predicted that the drug would reduce or abolish the effects of the stress on subsequent learning. Comparison of groups 5 and 3 show that this prediction was substantiated ($P = 0{\cdot}03$). This effect becomes even more striking when group 5 is compared with group 1, which learned the soluble maze without pre-exposure to drug or stress. The performances of these two groups did not differ significantly ($P = 0{\cdot}40$).

The experimental design allowed still another comparison from which additional inferences regarding the interaction of nitrous oxide and stress effects may be made. Group 4 was exposed simultaneously to stress and nitrous oxide prior to working on the soluble maze under the influence of the drug. To be consistent with the results reported in the preceding paragraph this group's performance on the soluble maze should not have differed from that of group 2, which learned the soluble maze while drugged and without previous exposure to drug or stress. Table III and Figure 2 show how very similar these performances actually were, the small differences not being statistically significant ($P = 0{\cdot}52$).

All these results indicate that, whenever the drug was combined with exposure to stress, subsequent learning of the soluble maze did not differ from learning of the same maze by corresponding control groups.

Discussion

Although the results described above have verified the hypotheses tested, their interpretation is not as straightforward as may at first appear. Maier (1949) and others have taken the view—

> ". . . that behaviour elicited during a state of frustration has certain unique properties, and that these properties make frustration-induced behaviour different in kind from that produced in a motivated state."

Among these "unique properties" are the symptoms of "anxiety." It may be, then, that the significantly better performance on the soluble maze by subjects receiving the drug during the stress period is interpretable in terms of the effects of the drug on these "unique properties."

However, at least one other possible interpretation of the present results seems worthy of consideration. Under the conditions which characterize the insoluble problem technique it is frequently the case that certain effects of practice on the first task transfer to practice on the second. Such transfer effects may facilitate or retard learning of the second task. It is possible that, in the present experiment, practice on the insoluble maze interfered with learning of the soluble maze, which would account for the significantly greater number of trials taken by group 3 in the soluble problem phase as compared with group 1. For example, one subject reported that he had tried out a method of approach to the insoluble problem, but, having

found it of no avail, made no attempt to use it for the soluble problem; this almost certainly prolonged his learning time for the soluble problem, since the method he had abandoned usually led other subjects rapidly to the solution. Again, the relatively rapid learning of the soluble problem by group 5 might have been due to the effects of the drug on learning during the insoluble problem. Comparison of groups 1 and 2 shows that learning with the drug is significantly slower than learning without the drug. It might be expected, therefore, that less learning took place when, as in group 5, subjects practised on the insoluble maze while under the influence of the drug than when the drug was not present, as in group 3. Previous research (McGeoch and Irion, 1952) has shown that the extent of learning of a first task is related to transfer effects which may influence learning of a second task, the effects increasing with increasing extents of original learning. Such being the case, it might be expected that group 5 would learn the soluble task with less interference than group 3, an expectation supported by the results. A second study in the present series is being designed to investigate this interpretation.

It might also aid in interpreting results such as those obtained in the present study if measures were taken of the physiological symptoms of "anxiety" as they are manifested during the various experimental treatments of the subjects. It is planned to incorporate such measures in future experimental designs.

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RELATION OF SKIN TEMPERATURE TO PAIN THRESHOLD

BY

K. R. L. HALL

From the Department of Psychology, Barrow Hospital, Bristol

Two recent studies have established a very close relationship between environmentally-induced changes in skin temperature and the pain threshold for radiant heat stimuli. The present experiment was designed to verify the relationship by using as pain test area the skin surface of the back of the hand, as the skin temperature here is likely to vary from individual to individual much more than for the forehead.

Subjects were 50 neurotic and depressed patients of both sexes, age range 20–79. Skin temperatures were recorded on the right hand until a steady level was reached, then radiant heat stimuli were applied to a blackened area of the left hand. Results showed a highly significant negative correlation between pain threshold and skin temperature level, while differences due to age and sex were negligible. Drops in skin temperature were a common reaction during pain testing.

The significance of this finding is discussed in terms of the adaptive function of skin temperature changes and their relation to tissue damage. The results are consistent with the view that the pain experience, although having its distinctive sensory components, is intimately related to, and affected by, central factors of autonomic regulation. Some implications for general experimental and clinical research on pain are briefly outlined.

I

Introduction

According to Hardy, Wolff and Goodell (1952), cutaneous pain thresholds determined by the radiant-heat method have a high degree of uniformity and reliability in man, provided that certain psychological and physiological conditions, which have been reviewed elsewhere (Hall, 1953), are adequately controlled. One of the physiological conditions was said to be the temperature of the skin in the area to be tested, and Hardy and his associates have tended to use the forehead as the stimulation area because the skin temperature in this region is reported by them to remain very constant in normal subjects (34° C. ± 0·5° C.) within certain limits of environmental temperature. Where the environmental temperature is outside these limits, they have proposed a correction formula enabling the experimenter to express pain threshold results in terms of standard conditions.

Recent experimental demonstration of the relationship between heat-pain threshold and skin temperature has been made in two studies. Birren, Casperson and Botwinick (1951) varied the skin temperature of the forehead area to be stimulated on their subjects by applying to it a cool or a warm water bag with dry surfaces. Then, within 5 seconds after the skin temperature had reached the required level, the inked area of the forehead was placed against the heat-pain apparatus and exposed to the stimulus. They studied three skin temperature levels (85°, 90° and 95° F.), obtaining thresholds for all three on the same group of nine laboratory trained subjects. The three levels of skin temperature resulted in statistically significant changes in pain threshold; that is, the higher the pre-stimulus skin temperature, the lower the stimulus intensity at which the subjects reported pain. Under standard environmental conditions, they found that the range of skin temperatures on the foreheads of fifty normal subjects was from 90·3° to 94·8° F., giving a range of 4·5° F.

The second experiment was made by Hardy, Goodell and Wolff (1951). The

pain threshold on the blackened skin of the forehead and the back of the hand of four subjects was measured in the usual way in a room at 26° C. Skin temperatures were measured with a radiometer prior to each test of pain threshold. The subjects then moved into a room at 8° C., and the skin temperature and pain thresholds were measured at intervals over a period of 1 hour. The subjects then returned to the room at 26° C., and the measurements were continued for two additional hours as the skin temperature returned to control levels. In a further experiment, the foreheads of the subjects were heated locally and skin temperatures were maintained at levels between 38° and 43° C. The results of these experiments showed that experimental cooling of the skin 10° C. led to an elevation in pain threshold of roughly 200 millicalories/sec./cm.². Conversely, heating of the skin 10° C. caused a lowering of the pain threshold by approximately the same amount. The relationship between levels of skin temperature and pain threshold is said to be characterized by a straight line passing through zero stimulus at a skin temperature of 44·9° C. Their results suggested that the skin in the areas tested must be raised to this temperature to be noxiously stimulated, regardless of the initial level of the skin temperature.

The relationship between skin temperature and accurately determined heat-pain threshold is sufficiently convincing for Whyte (1951) to suggest that radiant-heat pain thresholds should be measured and expressed in terms of skin temperature rather than amount of radiant energy. His comment on the Hardy-Wolff-Goodell technique, when used for measuring variations in pain threshold, was that it afforded an excellent method of determining peripheral blood flow.

In view of this very close relationship reported between experimentally induced changes in skin temperature and pain threshold, it was decided to carry out an experiment to try and verify the relationship by keeping the environmental conditions constant and using as stimulation area a region of the skin likely to show a reasonably wide range of inter-individual skin temperature variation. If the same relationship could be established on a group of subjects of both sexes and covering a wide age range, the uniformity of the pain threshold, under prescribed conditions, would be confirmed, with the implication for further experiments of being able to predict, from the resting skin temperature level, what the threshold point should be for any given individual.

Apart from variations in skin temperature brought about by environmental changes, it has been shown in a number of studies that the skin temperature of the extremities is reflexly susceptible to experimentally induced stress. Thus, Mittelman and Wolff (1939) reported considerable drops in the skin temperature of the fingers when "affective states" were induced in their subjects by discussion of personal difficulties. Helson and Quantius (1934), on the other hand, reported a predominance of increases in the skin temperature of an area of the face under similar experimental stress, but the discrepancy between the two sets of results may be due, not so much to the different skin areas studied, as to the fact that the latter authors do not seem to have allowed for the time lag required for the skin temperature to reach a reasonably steady level before beginning their experiments. Plutchik (unpublished survey) has suggested that the direction of change under stress will tend to vary according to whether an individual's "steady" level is initially high or low. As the expected reflex response to stress is usually a sympathetically mediated one of vaso-constriction, the hypothesis is here put forward for verification that a drop in skin temperature will be the most probable reaction to heat-pain stimulation, provided that sufficient time is allowed for the subject to achieve a skin temperature equilibrium in relation to the air temperature of the laboratory.

II

Method and Procedure

Subjects.

Fifty psychiatric patients (25 male, 25 female), diagnosed as showing some form of neurotic or depressive reaction. The cases were otherwise clinically unselected and untreated. The age range was from 20–79 years old, the distribution being 20 in age group 20–39, 20 in the 40–59 group, and 10 in the 60–79 group, the numbers in each group being equally divided between male and female. The mean age for the males was 45·4, S.D. 23·9; for the females 45·0, S.D. 24·4.

Apparatus.

Heat-pain stimulation from a 500-watt lamp was focussed through lenses so that the focal point coincided with the aperture at which stimulation was exposed. Stimulus intensity was controlled by a variac transformer, readings being made from a voltmeter connected to its output side so as to allow for possible A.C. mains fluctuations. Exposure-time of stimulus was controlled by a hand-operated camera shutter fixed between the heat-source and aperture. Routine calibration of the voltmeter scale with radiant heat intensity at the aperture was carried out with a radiometer.

Skin temperature was recorded by means of a thermistor, 1 inch by half inch in area, connected by long leads to a galvanometer, with a small potentiometer in the circuit for calibration adjustments to be made. The galvanometer scale was calibrated by plotting readings given by the thermistor in varying water temperatures against those from a 100° C. thermometer.

Procedure.

The patient was seated in the laboratory, with right hand and forearm resting horizontally on a table. An area about 1 inch in diameter on the back of the left hand was blackened with Indian ink and allowed to dry. The thermistor was placed on the back of the right hand, being secured firmly but not tightly by an adjustable rubber band. The galvanometer was then switched on, and readings taken every minute throughout the experimental session. The laboratory temperature varied very little during the period covered by the experiments, being 21° C. ± 2° C. The patient remained in a room of equal temperature for 30 minutes to 1 hour prior to the experiment. At the first stage of the experiment, no instructions were given, except that he was to keep his arm still while his temperature was being recorded. Thermistor readings prior to pain stimulation were recorded until an apparently steady skin temperature level was reached for the individual. This level was arbitrarily defined as the first reading to remain steady for 4 minutes in succession. The time required for such a level to be reached varied from person to person, but was usually about 10 minutes.

When the steady level was reached, the pain apparatus was switched on. The patient was told that he was going to be given a test of sensitivity, and that he was to report any sensation of which he was aware when the light from the apparatus was exposed on to his left hand. The on–off mechanism of the shutter was demonstrated to him. He was also told: "As soon as you feel any sensation that you could describe as at all painful or hurting, I want you to tell me and to describe it to me. This is not an endurance test, but a test of sensitivity." Stimulus exposure was always for 3 seconds, the interval between stimuli being 65 to 75 seconds. Stimulus intensity was started at 50 volts, then raised to 80 (these levels being below normal warmth threshold), then by steps of 10 volts to a maximum of 150, or to such a point lower in the scale at which the patient first winced or withdrew his hand from the apparatus. The first verbal report of pain (VRP) in most cases preceded the reaction point, and usually consisted of a description involving the terms sharp stinging, pricking, stabbing or burning. It appeared usually to be a quite distinct sensation, as opposed to the relative uncertainty of any previous sensations of warmth, mild tingling, or something just touching the skin. Thermistor readings were taken immediately after each stimulus-exposure was finished.

After the standard pain thresholds were obtained, a measure of the latency of the sensation of tingling or pricking arising from exposure of a stimulus of 100 volts was recorded on a stopwatch, the patient being instructed to say "Now" as soon as he was aware of this sensation.

The experimental session with each patient lasted between 25 and 35 minutes, skin temperature readings being taken throughout the period.

III

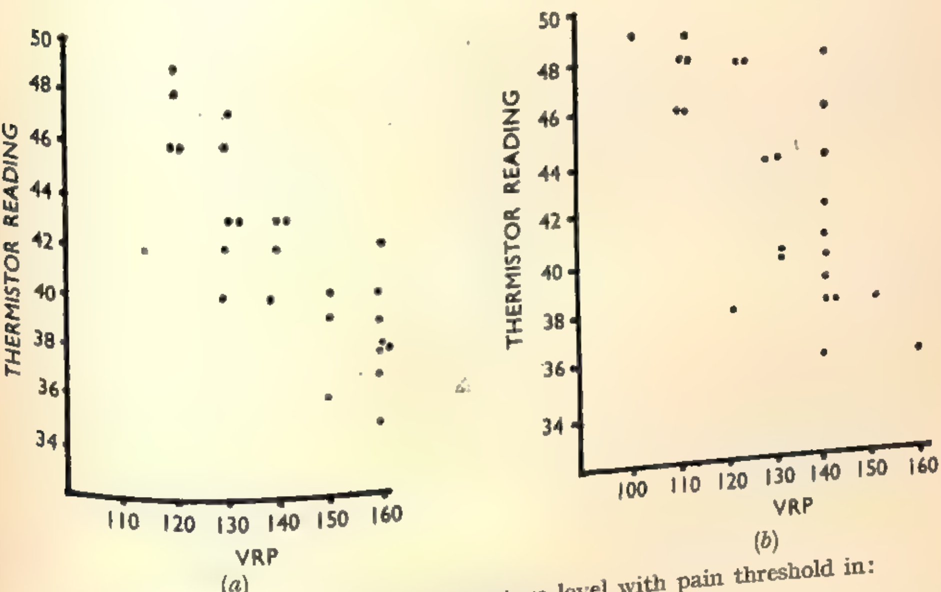
Results

Relation of resting skin temperature to pain threshold.

The range of resting skin temperature variation (8° C.) on the back of the right hand of this group of patients was, as expected, considerably greater than that obtained by Birren *et al.* on the foreheads of normal subjects (2° C.).

The main feature of the present results was the very close relationship between the steady skin temperature level of the right hand and the point at which the patient reported the first beginnings of a painful sensation (VRP) on the stimulated area of the back of the left hand. This relationship is shown in the dot diagrams of Figure 1, from which it will be seen that the correlation is somewhat higher for the male group ($r = -0{\cdot}83$, $P = < 0{\cdot}001$) than for the females ($r = -0{\cdot}63$, $P = < 0{\cdot}01$). Both groups show a highly significant tendency for low pain threshold to be associated with high skin temperature, and the reverse. The ordinate of the diagrams shows the thermistor readings obtained directly from the galvanometer scale, each unit being equal to 0·5° C.

FIGURE 1



Correlation of resting skin temperature level with pain threshold in:
(*a*) 25 male patients
(*b*) 25 female patients

The somewhat greater variability in the relationship for the female group may be due partly to the fact that they tend to show slightly more fluctuation in skin temperature during the experimental session, and partly that some of the females tend to give more anticipatory responses. Although the means and standard deviations for the resting skin temperature distributions are not significantly different as between male and female, there is a tendency for the females to have lower verbal and reaction thresholds. The difference in verbal thresholds is not statistically significant, while that for reaction is significant at approximately the 0·01 probability level.

The possibility that age might affect resting skin temperature and hence pain threshold was checked by comparing the skin temperature and pain values for

the 30 patients in the age range 20–49 with those for the 20 patients in the 50–79 range. No statistically significant differences were found.

The mean values for skin temperature (in thermistor units), verbal and motor response to pain (in volts), for the two sex and age groups are shown in Table I.

TABLE I

Group		N.	*Skin temperature*	*VRP*	*Motor reaction*
Sex ..	.. Male	25	41·3	142	153
	Female	25	43·0	130	139
Age ..	.. 20–49	30	42·3	134	146
	50–79	20	42·2	139	148

The two other measures recorded, the first report of any sensation such as warmth mild tingle, etc. (WPP), and the latency of the feeling of tingle-prick at 100 volts showed very little correlation with skin temperature. The correlation of WPP and skin temperature was well below the 0·05 significance level, while that of latency was just significant at the 0·05 level for the females but not for the males. The first perception of warmth, using this experimental procedure, is a difficult and uncertain point for most subjects, whereas the pain end-point is usually clearly defined and distinct. It is not possible to attach much importance to this negative result for the warmth sensation. The latency method of obtaining pain thresholds, that is, expressing the threshold in terms of the time taken for a defined sensation such as tingle-prick-stinging to arise at a fixed stimulus intensity, has been used successfully in two studies with carefully trained normal subjects (Gregg, 1951, and Birren *et al.*, 1951).

The conclusion to be drawn from these results is that resting skin temperature of the area to be stimulated is a variable of primary importance in the determination of heat-pain thresholds. Neither sex nor age difference would seem to have any more than minor significance for pain threshold variation where skin temperature is kept constant.

Skin temperature changes during pain stimulation.

Where a recordable change (0·25° C. or more) occurred in the skin temperature level during the pain threshold determinations, it was found in the present study that it was usually in the direction of decrease from the resting level. The distribution of changes for the 50 patients is given in Table II, the frequencies for the males and females being shown separately. Although there is a tendency for the females to show more pronounced decreases during pain stimulation, the difference in comparison with the males is not significant.

It will be seen that only 9 out of the 50 cases showed a measurable increase in skin temperature whereas 29 showed a decrease of 0·25° C. or more, and 12 showed no recordable change. The skin temperature level is likely to vary a little independently of any experimental stimulation, but the change is in most cases sudden enough in the present series to suggest that it is related to the experimental situation. In one case, a female patient, a pronounced decrease began to occur as soon as the pain procedure was started, and the skin temperature level had dropped 3° C. by the time she had reported pain. Her pain threshold in relation to her resting skin temperature level was consequently very high. It would appear that the

most common form of reaction to the pain procedure is one of vasoconstriction, as was to be expected from the findings of Mittelman and Wolff (1939). Where an increase occurred, it was usually very small and could have been due to the patient not having reached a steady level before the experimental stimulation was begun.

TABLE II

FREQUENCY OF CHANGES IN SKIN TEMPERATURE LEVEL DURING PAIN STIMULATION

	Type of change in degrees centigrade						*Total*
	+ 0·25 *and over*	0	− 0·25	− 0·5	− 0·75	− 1 *and over*	
Male ..	4	9	4	4	1	3	25
Female ..	5	3	3	7	3	4	25
Total ..	9	12	7	11	4	7	50

The suggestion made by Plutchik (unpublished) that a decrease in skin temperature, as a result of some form of experimental stress, is most likely to occur when the resting level is high, is not confirmed by the present findings. Dividing the resting skin temperature distribution into those with a thermistor reading of 42 and over (N = 27) and those with a reading of 41 and less (N = 23), there is no significant difference in the proportion of cases showing a decrease of 0·5° C. or more in the former group than in the latter. Regardless, therefore, of whether the initial resting levels are relatively high or low, if a change of any magnitude occurs at all it is likely to be in the direction of decrease rather than increase, of vasoconstriction rather than vasodilatation.

IV

Discussion

The dependence of heat-pain threshold upon the skin temperature of the area stimulated is a confirmation of the fact that, under carefully controlled conditions, the pain threshold, as distinct from reactions to pain, is relatively uniform from individual to individual when equation of skin temperature is made. In view of the closeness of the relationship that we have observed, particularly in the group of male patients, it seems probable that skin temperature should be treated as a variable of some potential significance in all psychophysical studies of cutaneous pain, whatever the method of stimulation. Possibly some of the variability in threshold for touch-pain reported by Rey (1943) may be due to differences in skin temperature as well as to special attitudes aroused in the subjects by the fact of taking part in a pain experiment. Similarly, it is a factor that might well be taken into account in assessing the conditionability of human subjects to some form of noxious stimulation intended to elicit a motor response. The generality of the relationship to other forms of noxious stimulation and other qualities of pain remains to be established by further experimentation, but there is some evidence from experiments by Bilisoly, Goodell, and Wolff (1954), in which hair-pull pain and von Frey hair stimulation were used, that pain thresholds for these stimuli are lowered in a vasodilatation zone of the skin.

It remains to inquire somewhat more closely into the possible mechanisms

involved in the skin temperature—pain threshold relationship, and to consider two of the possible explanations. The changes brought about in skin temperature by environmental alteration are known to be due respectively to vasoconstriction in the case of a decrease and to vasodilatation in the case of an increase in skin temperature. A theory such as Nafe's (1934) would have supposed that pain occurs at certain high or low temperatures as a result of reflex movements of the smooth muscles of the blood vessels supplying the warmth and cold receptors. This variety of the intensive theory of pain has, however, been refuted by a number of anatomical and physiological researches, one of which, carried out by Ebaugh and Thauer (1950), showed that, contrary to the well-marked effects on pain threshold, alteration of environmental temperatures had no effect on warmth and cold thresholds. Our own finding that the first perception of warmth has no significant relationship to skin temperature may be said to confirm this provisionally, although a much more exact procedure for warmth threshold determination will be required in further experiments.

Modern interpretation of the nature of cutaneous pain is very closely linked with the triple relationship of skin temperature—pain threshold—tissue damage, and the important experiments of Bilisoly *et al.* (1954) have done much to clarify the problem. That it is in fact the vasodilatation in the skin, and not the heating of the skin alone, which brings about a lowering of pain threshold, was deduced from some unpublished results of Goodell, Graham, and Wolff, quoted by these authors. Reflex vasodilatation was produced in the skin of the thorax and hand by immersing the lower extremities in water at 43° C. and simultaneously drinking hot fluids. Pain thresholds were found to be lowered in the area of reflex vasodilatation, even where the area was experimentally cooled by a draft of air from a fan. Bilisoly and his associates clarified the relationship somewhat further when they showed that lowered pain threshold, in an area of vasodilatation in the skin, was accompanied by increased tissue vulnerability. Thus, injection of a noxious agent (histamine phosphate), causing elevation of skin temperature about 1 to 1·5° C., not only resulted in a lowering of pain threshold but also in the production of significantly more skin damage with application of the same heat-intensity.

The relationship between elevation of skin temperature and lowered pain threshold is thus found to be associated with a greater vulnerability of the skin area to actual tissue damage. As a result of this and other experimental evidence, the pain threshold can be given a physical definition such as that of Hardy (1953), who says: "On the basis of the experimental evidence presented it seems desirable to conceive of noxious stimulation as resulting from reactions at the pain fibre endings (probably involving protein inactivation) at a rate so rapid that continued application will result in the destruction of tissue. . . . The threshold of pain and reflex responses to noxious stimulation by heating is determined by the lowest rate of inactivation of tissue proteins which will cause tissue damage if the thermal stimulation is sufficiently prolonged" (pp. 738 and 739).

The function of the relationship of skin temperature-pain threshold-tissue damage can presumably be considered to be a protective one for the organism. The adaptive value of the vasoconstrictive reflex as a sympathetically-mediated reaction to an emergency is obvious, and hence the finding that a drop in skin temperature was the most frequent change during pain stimulation was to be expected. The effect of more or less persistent states of emotional disturbance upon the skin temperature and upon the pain threshold needs further investigation, and there is no obvious correlation between resting skin temperatures and diagnosis of depression or agitation or anxiety in the present group of patients.

We may conclude that skin temperature, as a hypothalamically-regulated function, forms an essential link between the now well-established sensory characteristics of cutaneous pain and the supposedly cortical factors of conditioning and learning responses to noxious stimulation. The susceptibility of skin temperature to emotional and other influences may result in a direct facilitation or inhibition of the appreciation of a given stimulus as painful. The present results, and the previous work reviewed, are consistent with the view that the pain experience, although having its distinctive sensory components, is intimately related to, and affected by, central factors of autonomic regulation. Thus, a synthesis of the sensory and the emotional theories of pain seems necessary to fit the recent experimental data.

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THE RELATION BETWEEN HAND MOVEMENTS AND INTELLECTUAL ACTIVITY IN A SKILLED TASK

BY

M. A. VINCE

From the Psychological Laboratory, Cambridge

The experiment continues an investigation (Vince, 1953) into the relation between intellectual processes and hand movements in a sensori-motor task. Subjects were required to trace a repeating pattern of circles which appeared one, two or three at a time in a slit. Only one circle at a time could be hit and subjects were instructed to discover for themselves a good path for joining the maximum possible number of circles. The experimental situation thus allowed for various levels of response: pursuit movements, predictive movements, choice and a change of plan. At the end of the experiment subjects were asked to draw their idea of the pattern.

The results support others obtained earlier in showing that in this situation the subject's idea of the pattern he is tracing is determined by his total (motor, as well as visual) response to it. In addition, there was evidence that in some cases improvements occurred in the sensori-motor performance which were not reflected immediately in the subjects' idea of the pattern.

I

Problem

The problem may be stated as follows: learning a skilled task results in more than a complex sensori-motor habit. The skilled person differs from the unskilled not only in being able to make the correct motor adjustment at the right time, but also in his intellectual assessment of the particular situation he has learned to deal with. His idea of the situation is likely to be more correct and more detailed than that of the unskilled person. The question under consideration is, how is this idea of the situation built up in the course of learning?

In an earlier experiment (Vince, 1953) it appeared that the intellectual element in a particular learning task was a product not only of the objective situation but of the subject's activity in that situation.

In that experiment subjects were instructed to draw a line connecting up a number of circles arranged in a zig-zag pattern. The circles appeared one at a time in a slit, and without warning so that success depended on correct predictive movements; subjects had to respond to and at the same time learn to predict the circles as the pattern was not previously known to them. Afterwards, subjects were asked to draw their idea of the pattern, and it was found that distortions in this drawing were traceable to early "wrong" responses (mainly movements made in response to the single circles, or short parts rather than the whole pattern).

It was hoped to check this finding in a situation where it would be possible to relate a correct idea of the external situation with correct responses, as well as distortions of the pattern with "wrong" responses.

For this purpose the simple pattern which required a response for every circle was changed to a more complex one. It was hoped that in this way a situation requiring a simple form of adaptation could be changed to one requiring more choice, judgement, and also providing for a change of plan; it would then be possible to trace in more detail the relation between the responses made by the subject in the course of learning, and the idea of the pattern which appears to be built up from the total situation, including these responses.

II

Experimental Situation

The experimental situation was similar to that described in the earlier experiment. The subject sat facing a 6 inch vertical slit in a screen, in the slit was a single red circle. The subject was given the following instructions: "You look at the slit and you will see this red circle. The circle is filled in with red because it is the first circle in a pattern of circles, the rest are not filled in. When the drum is switched on the paper will move from right to left. You can move the pencil up and down the right hand side of the slit. What you have to do is try to draw a line connecting up as many of the circles as possible. Sometimes more than one circle will appear at a time, therefore it is not possible to hit them all. You have to work out what the pattern is like in order to hit as many circles as you can. The pattern does not last very long, and when another red circle appears, you will know that it is beginning again. The pattern is always the same. It will occur six times before the drum is switched off."

The pattern of circles appears in Figure 1, A (i), B (i), and C (i). It was drawn six times on a paper band which moved at the rate of 20 mm. a second behind the slit. Each subject was given 30 patterns in all to trace, there being a pause after every six patterns. During this pause subjects were asked for their introspections, and these were written down. At the end of the experiment they were given a roll of paper and were told it was identical with that on which the circles had been drawn. They were asked to take as much of the paper as they required and to draw on it all they could remember of the pattern of circles. When they had put in all the circles they could remember (circles in Figure 1, A (ii), B (ii) and C (ii)) they were given a coloured pencil and asked to put in any others they remembered only very vaguely (dots in Figure 1, A (ii), B(ii) and C (ii)). They were then asked to draw the line they had been trying to draw in order to hit the circles. Some included alternative routes, which are shown by a dotted line. There were 30 subjects in this group (I).

A second group of subjects (II) were given the same procedure, but did not draw the line connecting up the circles as they passed in the slit. The instructions were changed a little, these subjects being told to work out what would be a good path to take, if they had to draw a line connecting up as many circles as possible.

The requirements made of the two groups of subjects differed of course by more than the substitution, for the overt response of tracing the pattern with a pencil, of the non-overt response of watching the pattern to work out a good path. In order to hit the circles the first group needed to learn to predict their position and to move in advance to that part of the slit where a circle would appear; correct prediction required considerable exactness of timing and movement. The pursuit of circles which had already appeared was an unsatisfactory method. This becomes obvious from consideration of the motor record. What the second group was actually doing is less certain because there was no record, but it is suggested by their introspections that their understanding of the instructions and their technique in carrying them out varied much more, also they were not forced to predict the circles, nor were precise spacing and timing required of them.

There were 60 subjects in all. Of these, 30 were in group I (those who traced the pattern) and 30 in group II (those who merely watched). Of the 60 subjects 20 (10 in group I, Figure 1, A, and 10 in group II) were research workers, aged between 22 and 31 years; 20 (10 in group I, Figure 1, B, and 10 in group II) were students, aged between 20 and 34 years, and 20 were sailors (10 in group I, Figure 1, C, and 10 in group II) aged between 18 and 22 years.

III

Results and Discussion

(1) *Subjects who traced the pattern.*

In general, the subject's idea of the pattern reflects his path through the circles. Part of the data are given in Figure 1, A, B and C. In the first column (i) is each subject's best record out of his last six, and in the second column (ii) his drawing of the pattern. The results confirm those of the first experiment. The paths taken by the subjects through the actual pattern of circles varied considerably, but the

FIGURE 1

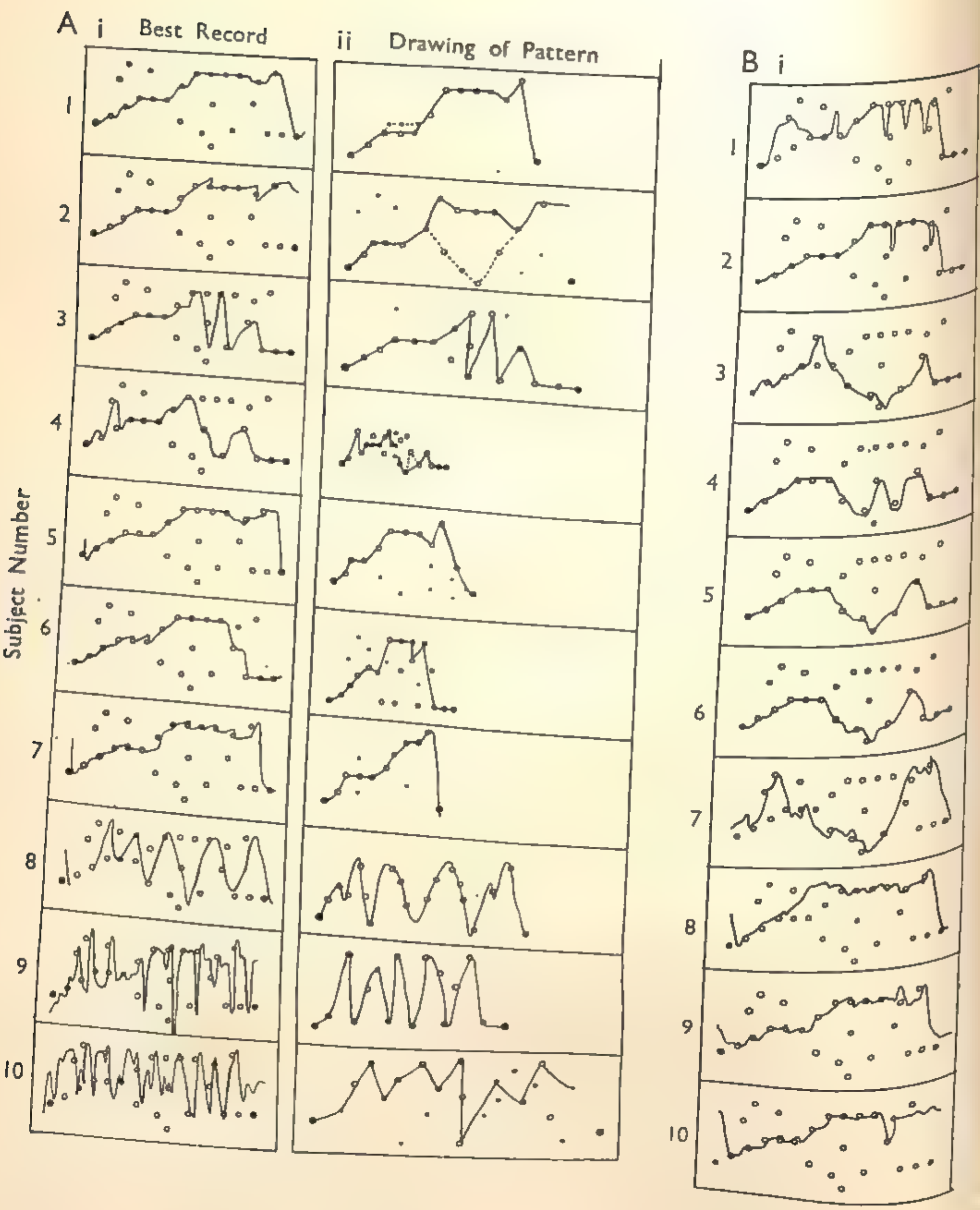
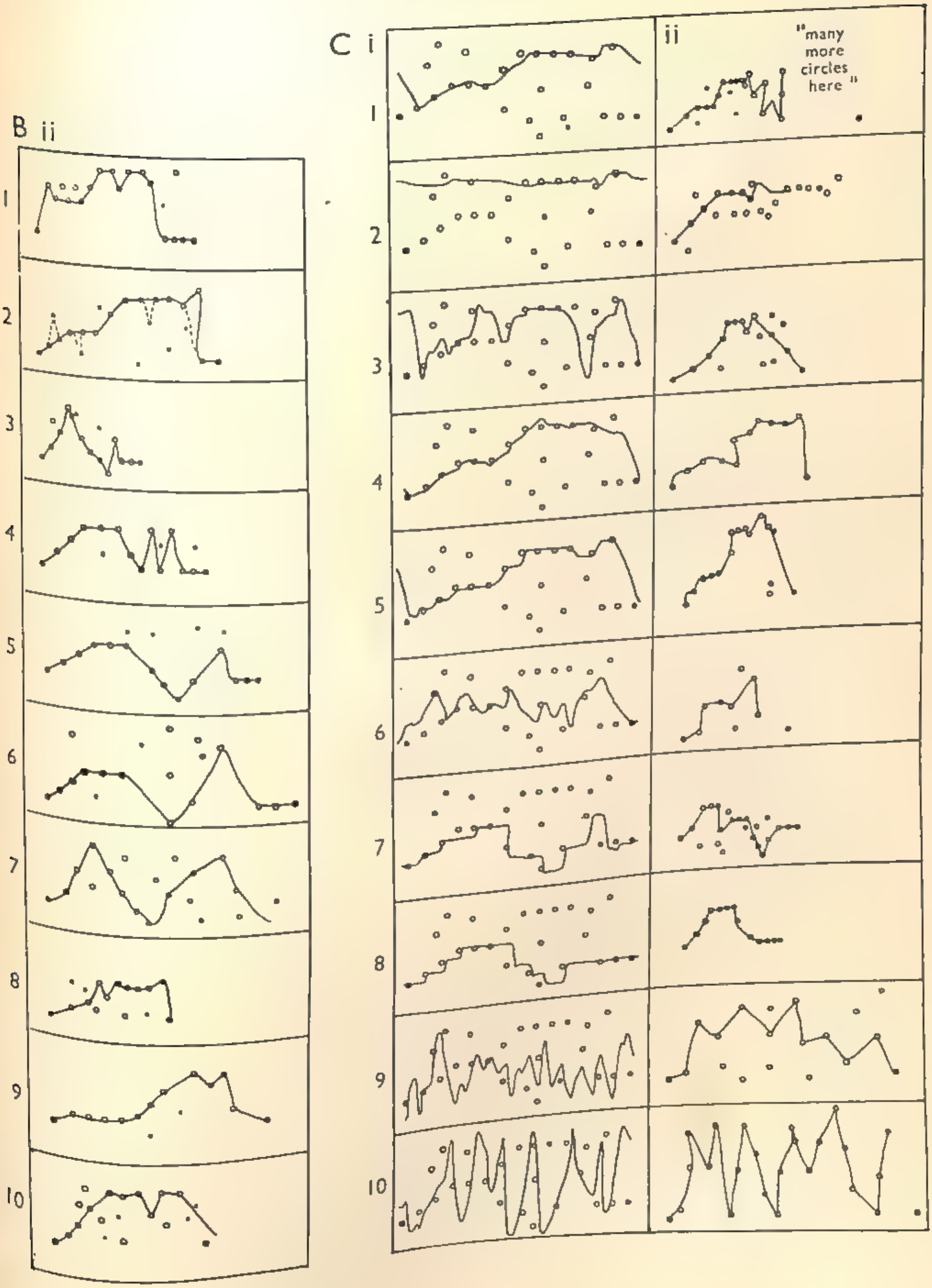

A i Best Record
ii Drawing of Pattern
B i
Subject Number
1
2
3
4
5
6
7
8
9
10

FIGURE 1



drawing always corresponded closely to this path, and subjects were rather unaware of or confused about the remaining circles. As in the earlier experiment where the pattern is actually distorted in the drawing it corresponds with the response made at the end of the experiment (Figure 1, A, Nos. 2 and 6; Figure 1, B, No. 10). Apparent exceptions to this occur in Figure 1, where there is a difference between the path taken in the final response and the drawing, but in these cases the path shown in the drawing may be traced to a path taken earlier by the subject, or it may represent a confusion of two or more paths taken at different times (Figure 1, A, Nos. 4 and 5; Figure 1, B, No. 8; and Figure 1, C, Nos. 1, 3 and 6). The earlier paths do not, of course, appear in the figure.

In this experiment there were six subjects who made correct predictive movements in parts of the pattern which they drew incorrectly, although these discrepancies were always small (Figure 1, A, Nos. 3, 4, 5 and 6; Figure 1, B, No. 3; and Figure 1, C, No. 5). The error in the drawing corresponds to an earlier stage in the development of their response. This suggests again that the subject's idea of the pattern reflects not only the actual pattern but also his response; or perhaps that in this task the situation can be changed, and so the idea distorted by the response. But in addition to this, as the learning progresses the response is corrected and is likely to be changed, and brought more and more into line with the pattern as it really is. When this happens the corrected response may have to be repeated several times before the idea is clarified. Before investigating this point in more detail, however, it is necessary to consider the problem which arises from it. How does the capacity to make correct predictive movements arise, if it does not depend initially on a correct idea of the pattern?

The process of learning to trace the pattern of circles. At the beginning the majority of the subjects simply pursued the circles as they appeared in the slit, either selecting a limited number (usually the nearest ones) or trying to pursue them all. The other subjects either rarely or never pursued a circle but responded in what appeared to be a random fashion by drifting up and down in the slit. As the circles are fairly evenly distributed this method resulted in their hitting or narrowly missing at least a few of them.

Nine of the thirty subjects did not learn to predict. Six of these continued with their initial mode of response (pursuit) until the end of the experiment (Figure 1, A, Nos. 9 and 10; Figure 1, C, Nos. 7, 8, 9 and 10); the other three (Figure 1, A, Nos. 6 and 7; and Figure 1, B, No. 7), who had all begun with very rapid pursuit, tended to slow their movements down after a few records. But their responses were rather variable and appeared to conform less with the position of the actual circles than with a general idea of the pattern. This general idea of the pattern was in line with their responses, for example, one of these subjects (Figure 1, A, No. 8) said afterwards: "I know it was an up and down pattern, because I had to do that." The drawing represents this general idea.

However, twenty-one subjects did learn to predict, although some did not learn the whole pattern. A few of these (Figure 1, B, No. 8; Figure 1, C, Nos. 3, 4 and 5) resembled the group last described, and relied on a general plan. These adapted their original path to bring it more into line with the actual pattern. Where (i) and (ii) differ in the first three, the drawing represents an earlier path.

But for those subjects who learned to predict the most usual mode of development was different from this. In their early records they pursued individual circles or drifted through parts of the pattern. A route based on pursuit or drift tended to vary a little, but not much, from record to record. A stereotyped path of pursuit movements was then built up, following one or more recognizable points in the

pattern, such as the red circle marking the beginning. As this stereotyped path grew from record to record, at the same time the pursuit movements tended more and more to become predictions. The process of change from pursuing to predicting a circle varied, sometimes it occurred in one step, sometimes the movement was moved forward gradually over a series of records. This path of predictions grew gradually longer until the whole path was predicted, or until the end of the experiment.

It seems likely that the shifting forward of the responses in this change from pursuit to prediction is a fairly automatic process (i.e. determined by the sensori-motor sequence) and that there is not necessarily a detailed grasp of the pattern directing it. The argument against this is that there were subjects who pursued a fairly stereotyped path of circles without tending to predict them at least more than a few times. But there is evidence for it in other experimental situations —it has to be guarded against in reaction-time experiments, for example. In addition, subjects could predict repeatedly and correctly and still make a mistake in the drawing. Also the shifting forward of the responses did not stop with the change from pursuit to prediction. Part of the responses of all those subjects who at any time learned to predict were also at some time shifted too far forward and so errors of anticipation were made. With many subjects this type of error occurred repeatedly. Mostly these errors were recognized and corrected, but for one subject (Figure I, A, No. I) repeated anticipations appeared to be responsible for a change in the shape of the path finally taken. And again, two subjects (Figure I, A, No. 8; Figure I, C, No. 10) who had begun rather slowly to build up a path of predictions had this path broken down, apparently by making anticipations which they did not correct.

Of the twenty-one subjects who learned to predict, one (Figure I, C, No. I) adopted a method which was unique in this experiment. But it throws some light on the learning process and may be described in detail. He began with drift movements which seemed entirely unrelated to the actual pattern of circles. He drew two, three or four peaks during each pattern. After about twelve patterns he began to predict a few circles following the red circle, and after that added a few more predictions every now and then until the end of the experiment, thus leaving less and less space for his drift movements ("I get the ones I know, and try to remember where the next one is"). By the thirtieth record (the last) he had learned to predict as far as the final circle in his particular route, but not as far as the red circle marking the beginning of the next pattern. His drawing is interesting, being almost correct, but including at the end of the pattern a long stretch of imaginary circles joined by a line reminiscent of his drift movements.

The predictions of this subject had been preceded in earlier patterns by pursuit movements in one or two cases only; and in this respect his method differed from that of the other subjects who learned to predict, and who only rarely predicted a circle which had not previously been pursued. But this difference is probably less important than the condition which provides for prediction and which is the same for all subjects: an already stereotyped response to some part of the pattern. It is possible, once the situation is stabilized by this stereotyped path that the response of noting the position of an extra circle and that of pursuing an extra circle are fundamentally similar.

In addition to this subject there were eight who managed to change, usually to improve, their route, in the course of the experiment (Figure I, A, Nos. 2, 3, 4, 5 and 6; Figure I, B, Nos. I, 2 and 9). These had all begun by pursuing the circles, in most cases the lower ones. The change usually took place at a point in the

pattern where two circles appear simultaneously, and never before the path of predictions had been stereotyped at least to the point where the change occurred. Subjects who responded in a variable rather than a stereotyped way did not provide themselves with the information on which to base a decision of this kind. After the change was begun the new path was completed in the usual way, by pursuing and then predicting the new circles. The method of subject No. 1 (Figure 1, C), the few cases of predictions occurring where there had been no previous pursuit movement and the subjects who changed their path all provide instances of the idea of the pattern preceding the response. But that even here the idea of the pattern also depends on the level of the response is shown by the conditions which provide for this kind of prediction—a stereotyped path of responses up to the point where it occurs.

The development of the subject's idea of the pattern. Six of the most successful subjects, including four of those who changed the path of their responses, were able to predict correctly and sometimes had done so repeatedly and still drew the pattern incorrectly, although discrepancies of this kind were small. These cases show that a subject could predict correctly when his idea of the pattern was not entirely correct.

This cannot satisfactorily be explained away by saying that between tracing the final record and drawing the pattern (which he did a few seconds later) some subjects had simply "forgotten" the pattern. One reason for this is that an error in the drawing could always be traced in the motor record. For example, one subject (Figure 1, A, No. 4), who had predicted all the circles correctly in his final record, drew a pattern in which the middle section was expanded and included two paths which he had taken at various times in his earlier records. Also for two of these subjects the error in the drawing had been anticipated in the introspections.

But it could be explained if the building up and consolidation of the responses were regarded as a process determined mainly by the sensori-motor sequence and the idea of the pattern as an abstraction from this. At the beginning this is a rather vague and general impression drawn from two sensory modes—visual and kinaesthetic. Sometimes it is confused by the subject having taken two or more different paths. It is clear from the results that correct predictions can be built up without the subject having an idea associated with them which is correct, circle for circle. Also that once the stable sequence of responses is built up a repeated visual impression may be sufficient for adding a new prediction to it. What is not clear is exactly when a correct idea is associated with correct prediction. In general, predictions which have been made several times correctly are more likely to be associated with a correct idea than predictions which have been made only once or twice, but there are exceptions to this. It is possible that the tendency to anticipate plays some part in the clarification of the idea of the pattern. If the performance is already fairly good the subject can correct such errors immediately and learn to avoid them.

It seems that the responses play a part in changing and perfecting the idea of the pattern. Again, this process needs detailed investigation, but an example may be given. In records 3, 4 and 5 one subject (Figure 1, A, No. 5) predicted correctly the three circles in the first horizontal part of the pattern; in record 6 she anticipated a subsequent downward movement and so only predicted two circles where she had previously predicted three. This error is reflected in her introspections after record 6 ("about the middle there are two circles horizontally together"). After this she sometimes predicted three circles here (in line with the actual pattern and her earlier responses) and sometimes two (in line with her idea). The idea persists in her drawing, but she was uncertain about it at the time and afterwards when shown the actual pattern said: "I *knew* there were three, I got them usually."

When the responses vary in this way and the idea is wrong it looks as if the process of correcting the responses must result ultimately in correcting the idea.

Introspections have been quoted, but their value seems to be limited. They often appeared to be throwing some light on the learning process, but on inspection of the records are shown to reflect earlier activity; no subject announced a plan of action *before* carrying it out although occasionally subjects who had built up a stable path mentioned extra circles they would like to add to it. If the present interpretation is correct, then introspections be relied on only to the same extent as the drawing.

The evidence suggests that there is no reason for believing that the subjects picked up an idea of the pattern from the appearance of the circles alone, and then tried it out in practice. But many subjects believed they were doing this. Introspections show that some were worried by having to watch the circles and respond at the same time and would have preferred watching only. In addition, at least two subjects deliberately refrained from responding for the length of one record or more, in order to work out more closely what the pattern was like; but this time seems to have been largely wasted, as the subsequent records were not essentially different from the earlier ones.

(2) *Subjects who only watched.*

The results do not indicate, of course, that it is impossible in this situation to obtain an adequate idea of the pattern without making a motor response. Thirty subjects were given the same task as the main group, but they were told to watch the circles only, and to try to work out what would be a good path to take to join them. Their drawings compare well with those of the first group but are certainly no better. An experimental situation which allowed a detailed analysis of the differences that occur under these two sets of conditions might throw some light on the role of the response in the learning process.

IV

Conclusion

It is the dependence of the subject's idea of the pattern on his activity which has been stressed, but this does not suggest that it is an exclusively one-way process. There is a very complex relation between the actual situation with the subject's response to it, and his idea of the pattern he is trying to trace. It needs more detailed investigation, but the general outline is clear; at the beginning the perception of the separate circles and the response to them give rise to a sensory impression from these two sources—vision and kinesthesis. There is a tendency to repeat, not exactly but roughly the same path in subsequent records. It is not known whether the repetition depends on this early impression of the shape of the pattern or whether it is a more automatic process including the pattern of movement; it is probably both. But the sensory impression is undoubtedly deepened by the repetition. It is at this stage that the mode of development begins to diverge. For a few subjects the rather general idea of the pattern becomes the determining factor and directs their response. These were the unsuccessful subjects and their drawing of the pattern reflects their initial responses rather than the actual position of the circles. In the case of the successful subjects the building up and consolidation of the predictions may be a fairly automatic process, but the idea of the pattern is built up at the same time, although not apparently at the same rate. The question as to why there sometimes is and sometimes is not an idea to correspond with a

correct response (or an incorrect one) is not answered. But certainly when the idea was detailed and correct the responses were stable, and more important, adaptable (could be changed, or adapted to include an extra circle); and when the idea was wrong, or confused, the responses were less stable, but because the actual pattern was always the same, an error provided the evidence needed for correcting the response and, ultimately, the idea.

I am indebted to the Medical Research Council for financial support.

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SOME OBSERVATIONS ON THE OCCIPITAL ALPHA RHYTHM

BY

JAMES DREVER

Department of Psychology, University of Edinburgh

In order to test the hypothesis that the behaviour of the alpha rhythm is related to the kind of imagery used by different subjects, and in particular that the disappearance of the alpha rhythm during mental work is associated with the use of visual imagery, occipital EEG records were taken from groups of early blind, late blind, and sighted subjects during the performance of two spatial tests. Since the test scores differentiated between the groups it was argued that the performance probably involved a visual component. When the subjects were classified into the three suggested alpha-rhythm types, M, R, and P, the groups so obtained did not differ significantly from one another in terms of test scores. This was regarded as negative evidence so far as the hypothesis under investigation is concerned, especially since the alpha rhythm type supposedly associated with prevalently visual imagery was found most frequently among the blind.

It is clear that blindness is a complicating factor here as the amount and amplitude of alpha rhythm found in the blind records was less than in the case of the normal subjects. This, however, does not seem to invalidate the argument, but rather constitutes a problem for those who claim that absence of the alpha rhythm is associated with visual imagery. The hypothesis under investigation seems at present to be too simple and to need some reformulation before it will fit the facts.

Golla, Hutton, and Grey Walter (1943) have suggested that the occipital alpha rhythm from different subjects may be tentatively classified into three types, M, R, and P. The M or minus type of record shows very little activity at the 8–13 cps. frequency and what there is tends to be of low amplitude. The R or responsive type shows the "classical" alpha pattern. Here the rhythm is present when the subject is resting with eyes closed, but is blocked or much reduced by sensory, particularly visual, stimuli, by emotion, and by mental work. Lastly the P or persistent type shows an alpha rhythm of average (10–50 μv) amplitude, which is relatively difficult to block.

Associated with these differences in alpha rhythm, they suggest, are differences in prevalent imagery. The visualizer, presumably since his occipital lobes are constantly active, tends to produce the M pattern. The P type on the other hand is found in subjects whose imagery belongs predominantly to the auditory-kinæsthetic modalities, while the R type is found in subjects whose imagery is mixed.

In establishing their subjects' characteristic type of imagery the investigators supplemented the traditional introspective report with a record of their subjects' breathing, following a study by Golla and Antonovitch (1929) in which it is claimed that visualizers breathe regularly during mental work, but vocalizers do not. With a group of 60 subjects they show an association between EEG and imagery type of the kind predicted.

Further exploration of the possibility that the EEG may be used in this way to indicate psychological functions has been carried out by Short (1953) and Short and Grey Walter (1954). Short's investigation served mainly to consolidate the original findings. He used 150 subjects and asked them to carry out a variety of tasks such as multiplication, recalling verbal material, and solving mentally a spatial problem. EEG and breathing records were taken, and an introspective report asked for at the

end of each task. Significant associations were found between imagery type, respiratory type, and alpha type, with the qualification that two-thirds of the subjects belonged to the mixed or R type.

In the second investigation an attempt was made to get away from dependence upon introspective reports. Subjects were given spatial exploratory tasks to perform in which they had to draw and recognize figures outlined by means of a groove which they could follow with their fingers. A detailed analysis is given for 20 subjects. It is claimed on the basis of this analysis that the M and P types showed themselves to be significantly more efficient than the R in that they gave more correct answers and took less time to arrive at them. As an explanation for the result it is suggested that consistent vs. fluctuating imagery is the crucial variable.

While designing an investigation into the space perception of blind and sighted subjects and the possible effects of longer and shorter periods of early vision (1955), it appeared to the writer that there might be an opportunity to test the hypothesis relating imagery to EEG. In particular, if spatial problem solving proved significantly more efficient among those who had enjoyed vision longest, it might be assumed that this was because of some visual component in their performance, and if such a superiority was accompanied by differences in the alpha pattern the hypothesis would be supported. The advantage of the proposed experiment lay in the fact that both the variables were objective, and that no appeal to introspection, either direct or by way of a previously established connection with breathing, should be necessary. Accordingly bilateral occipital EEGs were taken from all subjects throughout the experiment. Two channels of an orthodox four-channel encephalograph were used feeding into a Marconi pen unit. In view of the large number of short and irregular resting/working periods analysis of the records was carried out by means of calipers and ruler. Each record lasted 30–40 minutes, during which time the subjects carried out a number of tasks. Performance was of a kind that could be scored, and the scores were related to the duration of the subjects' visual experience.

Subjects and procedure.

Thirty-seven blind and 37 sighted subjects were used, the two groups being matched for age, sex, and intelligence. In addition the blind subjects were divided into two further matched groups containing 19 subjects who had lost their sight before the age of four, and 18 subjects who had become blind since the age of four but not during the two years preceding the investigation. In the case of the blind subjects an adaptation of the Terman-Merrill scale was used, and in the case of the sighted a Moray House group test. Details are given below.

	No.	*Mean Age*	*Mean I.Q.*
Blind	37	14 y. 11 m.	111·7
Sighted	37	15 y. 3 m.	116·1
Early blind ..	19	14 y. 11 m.	112·2
Late blind ..	18	14 y. 10 m.	111·05

The age range was 9 y. 8 m.—19 y. 6 m. in both groups.

The I.Q. range was 88–154 for the blind and 90–150 for the sighted. Only one subject had become blind as a result of damage to the CNS; removal of a cyst involving injury to the optic tracts and subsequent degeneration; but she showed no EEG or other neurological abnormality. Detailed case-histories were available for the blind, and expert advice was sought in estimating the age at onset of blindness.

While doing the tasks the subjects reclined in a chair with adjustable back and leg rests. Sighted subjects had pads over their eyes, and the experiment took place in a sound-screened room. Resting records were obtained from each subject at the beginning and end of each session and between tasks. In view of the finding (Jasper, 1937; Lindsley, 1950) that anxiety may reduce or inhibit the alpha rhythm, great care was taken to put the subjects at their ease. The blind children were familiar with the experimenter's voice since he has had to do with the blind school for a number of years. Many of the sighted children came from the experimental school attached to a teachers' training college and were familiar with the test situation.

The first task was a figure-recognition test devised by Worchel (1951). In it the subject was given two small wooden blocks one in each hand. He could manipulate these as he liked so long as he kept his hands apart. He was then given, one after the other, four larger blocks, and had to say which of these could have been made if he had placed the first two blocks together. There were seven such series and the answers were either right or wrong. The important differences found were those between the blind and the sighted and between the early and late blind.

	Mean Score	*S.D.*	*N.*
Blind	4·8	1·49	37
Sighted	6·1	0·62	37
Diff. = 1·3 ..	$t = 4{\cdot}4$	P<0·01	
Early blind ..	4·1	1·52	19
Late blind ..	5·6	1·08	18
Diff. = 1·5 ..	$t = 3{\cdot}41$	P<0·01	

In addition to the quantitative data the subjects' reports sometimes suggested that they were trying to use visual imagery. In addition several late-blind subjects moved the two problem figures this way and that in the space before their now sightless eyes as though trying to *see* how they might go together.

In the second test the subject had to follow a pattern of pegs on a square peg-board, touching them one at a time with the forefinger of his guided right hand according to a set sequence. The peg-board was then rotated through 180° and all pegs except the last to be touched were removed. These were given to the subject, who had to replace them in the holes they had originally occupied. Errors were measured in inches from the correct hole. On this test the performance of the late blind was significantly better than that of either the early blind or the sighted. The difference between the two blind groups is the important one for our purposes. Reasons are given in the paper on early learning, to which reference has been made, for holding that the poor performance of the sighted group was due to lack of skill in dealing with tactile-kinaesthetic cues. It seems reasonable to suppose that the difference between the two blind groups was due to the fact that the late blind had continued their visual learning for a longer period, and that they were making use of some sort of visual skills in their solutions.

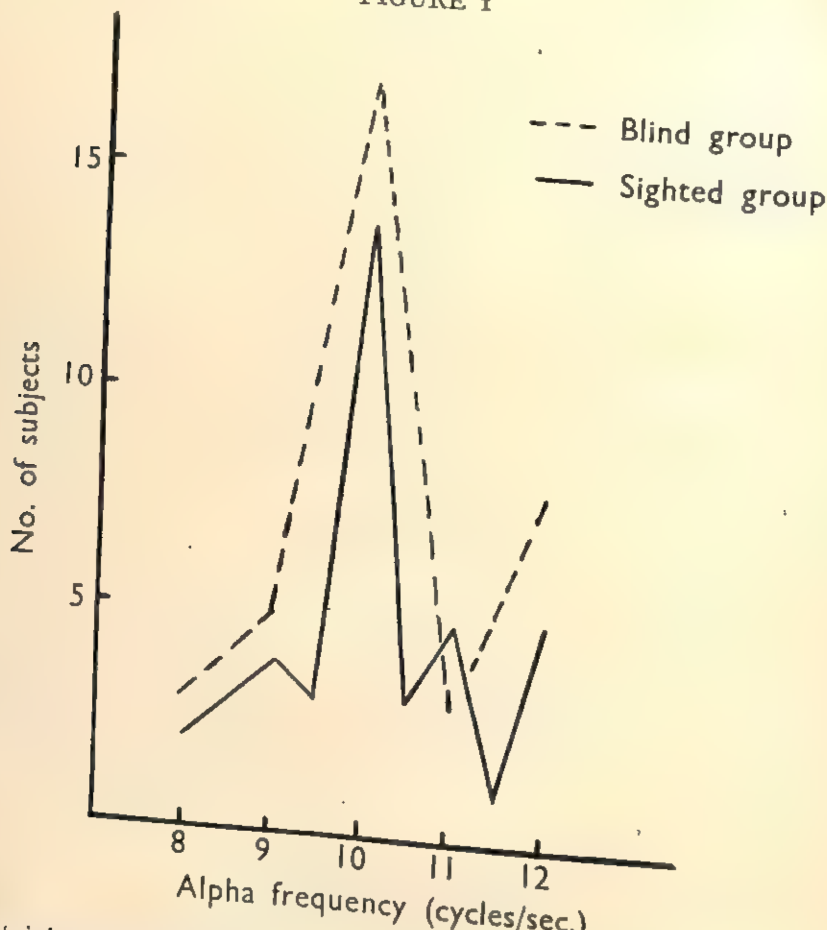
	Mean Score	*S.D.*	*N.*
Early blind ..	39·6	13·1	19
Late blind ..	26·2	11·9	18
Diff. = 13·4 ..	$t = 3{\cdot}46$	P <0·01	

Results

Consideration of the EEG records may be divided into two parts: (1) an overall comparison between the three groups, and (2) a comparison based on differences of performance in the two tests.

(1) A comparison of the alpha frequency between the blind and sighted groups (Figure 1) shows a very close agreement. We are clearly dealing with the same phenomenon in either case.

FIGURE 1



In obtaining the data for Figure 1 all subjects were included except one early blind, whose record did not contain anything that could be identified as an alpha rhythm. Raising the criterion for inclusion to an alpha percentage of 5, 50, and 75 in the resting record eliminates many more blind than sighted subjects.

	Early B.	*Late B.*	*Sighted*
More than 5 per cent. alpha in resting record	11	10	36
,, ,, 50 ,, ,, ,, ,, ,, ,,	5	6	31
,, ,, 75 ,, ,, ,, ,, ,, ,,	2	2	22

There is thus a marked difference in the amount of alpha present in the blind as compared with the sighted records even though the frequencies are the same. On the other hand there is no difference between the early and the late blind in this respect.

In examining the average amplitude of the alpha rhythm it was decided to use only those records in which it occurred during 5 per cent. or more of the resting periods. An average derived from a few short bursts would not be very reliable. A comparison on this basis is shown below.

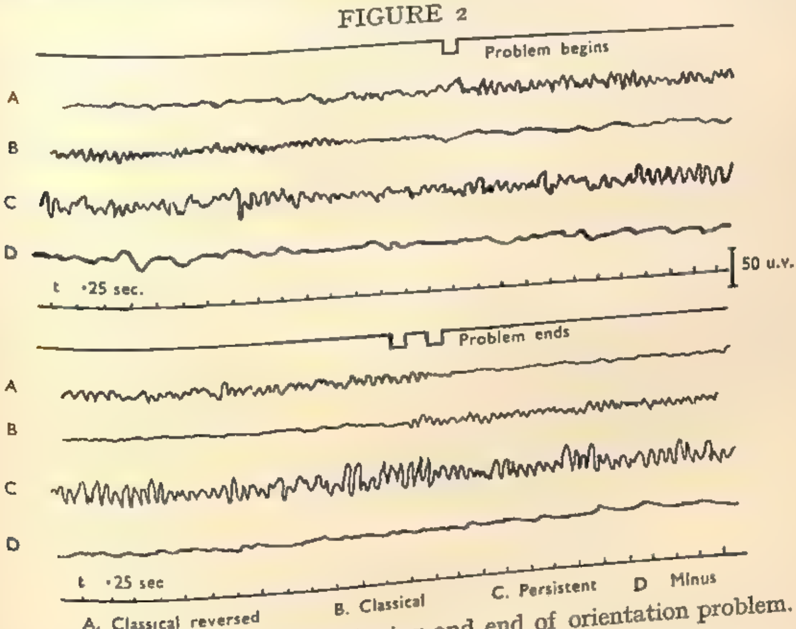
	Average amplitude	*S.D.*	*N.*
Early blind ..	12·4 µv.	—	11
Late blind.. ..	13·5 µv.	—	10
Total blind ..	13 µv.	3·46	21
Sighted	19·2 µv.	8·35	36

Diff. (blind and sighted) = 6·2 µv. $t = 2{\cdot}665$. $P < 0{\cdot}02$.

The difference between the two blind groups is not only not significant, it is virtually non-existent. If, instead of the average alpha amplitude, we take the mean of the maximum amplitude from each record, and include all records except the one early blind mentioned above, then the figures for the blind and sighted groups are 18·9 µv and 30·3 µv and the difference is significant at the 1 per cent. level.

Turning now to the threefold classification of alpha types that has been suggested it was found possible to identify most of the records as falling within the M, R, or P categories. Some borderline cases were observed, but the records did not give the impression of belonging to a continuum arbitrarily broken at two points. In addition, however, there were three anomalous records. Two of them might have escaped notice, though in both there was more alpha present during the tasks than during rest.

FIGURE 2



Types of alpha rhythm at beginning and end of orientation problem.

The third showed on the second or orientation test a rather striking reversal of the classical type. A tracing from this and three other records at the beginning and end of one of the items in the orientation test is shown in Figure 2. The subject whose record is described as classical reversed was a 12-year-old sighted girl.

Blindfolded and relaxed she produced a record in which low-level potentials were occasionally broken into by a burst of alpha. At the beginning of each item in the orientation test the alpha became continuous and remained so until the end, when it subsided. The relationship was not so well defined in the figure-recognition test. The alpha also tended to appear with auditory stimulation, but was absent when the eyes were open.

Omitting the three anomalous records the distribution of subjects by alpha type was as follows:—

	M	*R*	*P*
Early blind	10	9	1
Late blind	9	6	1
Sighted	3	20	12
Totals	22	35	14

One cannot conclude at this stage that the suggested association between alpha type and imagery must be mistaken on the grounds that most of those whose EEG pattern would make them visualizers are in fact blind. Prolonged disuse of the visual projection areas may have complex nutritional and other consequences. It seems clear, however, that the hypothesis under investigation has been formulated rather too simply. Added force is given to this criticism by the fact that of the three early blind subjects about whom it could be stated with confidence that they had never seen two belonged to the M type and one to the R with a low-level alpha of 10–15 μv. Similar results have been reported for blind subjects elsewhere (Baudouin *et al.*, 1939), and the original report by Adrian and Matthews (1934) that the blind show no alpha rhythm may be accounted for by the fact that they used only three subjects.

(2) In considering the relationship between EEG record and performance on the tests it is necessary to distinguish between an alpha-type classification based on each record considered as a whole and one which is confined to the record made during either the figure-recognition or the orientation tests. The alpha rhythm was more sensitive to the figure-recognition than to the orientation test, and if a classical response was going to appear at all it appeared there. This may be related to the behaviour reported above where there was a tendency to manipulate the objects before the eyes, and to the Adrian and Matthews finding that "trying to see" abolished the alpha rhythm.

If we group figure-recognition test scores by alpha type we get the following averages:—

Alpha type ..	*M*	*R*	*P*
Mean score	5·38	5·33	5·86
N.	22	35	14

The differences are not significant, but it may be worth noting that the same trend occurs here as in the rather similar test used by Short and Grey Walter in that the classical type does less well than the other two. Here, however, the explanation in terms of consistent/inconsistent imagery cannot plausibly be advanced. In particular, as has been pointed out above, the suggestion that M-type subjects are consistent visualizers is impossible to reconcile with the large number of blind subjects in the

M group. A more profitable line of explanation may come from recent work on the cholinesterase concentration in different parts of the cortex and its relation to patterns of adjustive behaviour (Krech *et al.*, 1954).

During the orientation test there was a greater tendency for the alpha rhythm to persist, and consequently the numbers grouped under each type show a gain in P at the expense of R.

Alpha type ..	*M*	*R*	*P*
Mean score	37·3	39·2	38·6
N.	22	25	24

In view of the very small differences it would be misleading to make anything of the fact that the trend is again in the same direction.

The only general conclusion which it seems possible to draw from a consideration of performance on the two tests is a negative one. When scores are classified in terms of the amount of visual experience that subjects have had, then the figure-recognition test discriminates between the blind and the sighted groups as well as between the two blind groups, and the orientation test discriminates between the late and early blind. It is difficult to avoid the interpretation that some kind of visual function is involved in the performance. On the other hand, when scores are classified in terms of alpha rhythm type, none of the differences between groups is significant. Even if we allow for complicating factors that may be introduced by the use of blind subjects it would seem that the proposal to relate alpha type to visual or other imagery cannot be accepted in the rather simple form in which it has been made.

The writer would like to express his thanks to the headmasters of Craigmillar, Moray House, and George Heriot's schools for providing subjects, and to his co-experimenter Timothy Regan.

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MISCELLANEA

A MODIFIED FORM OF WHEATSTONE'S STEREOSCOPE

BY

G. C. GRINDLEY

From the Psychological Laboratory, Cambridge

An apparatus based on the principle of Wheatstone's mirror stereoscope is described. The main changes introduced are (*a*) that by the use of semi-transparent mirrors it is possible to arrange that the subject's eyes are correctly converged and accommodated at the beginning of each exposure, and (*b*) that the times of exposure of the two figures can be varied independently.

I

Introduction

The present instrument is designed to overcome two of the difficulties encountered with most lens-type stereoscopes, namely that the distance apart of the eye-pieces must be adjusted for the inter-pupillary distance of each subject, and the related difficulty of arranging fixation for both eyes in the pre-exposure field.

These difficulties, it is claimed, can be avoided by using the well-known principle of the Wheatstone stereoscope (Wheatstone 1838), but with semi-transparent mirrors instead of the usual fully reflecting ones—thus allowing a common fixation point to be seen *through* the mirrors. The arrangement also makes it very easy to vary, quite independently, the moments at which each figure is switched on or off.

The Wheatstone principle has the obvious disadvantage that it requires a much bigger piece of apparatus than most lens-type instruments, but as the aim was to study in detail the effects of applying different stimuli to exactly corresponding points of the two retinae, it was felt that the extra size was worth while.

II

Description of Apparatus

A semi-diagrammatic section of the apparatus, at the level of the subject's eyes, is given in Figure 1.

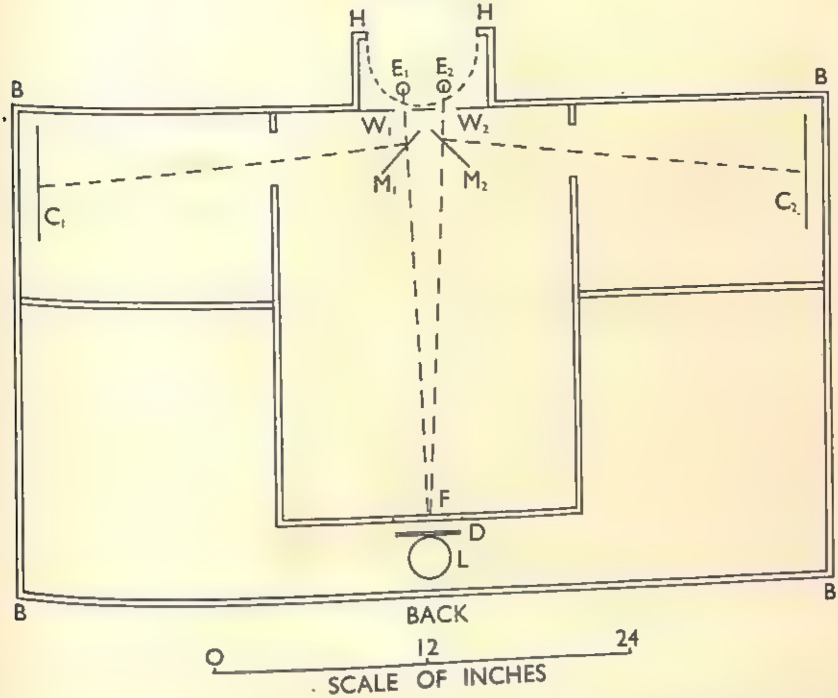
The subject sits with his head resting against a binocular eye-piece HH which projects in front of the main apparatus. This eye-piece is merely a wooden box, measuring about 8 in. × 4 in. × 4 in., open in front and cut so that with an edging of black velvet it is comfortable for most people's heads (whether they wear spectacles or not), and does not admit enough stray light to interfere with what is being shown in the stereoscope. This type of eye-piece is convenient because it also acts as a fairly steady head-rest, while allowing the subject (whose nose and mouth are below it) to give an unimpeded "running commentary" on what he thinks he sees. At the back of the eye-piece are two windows W_1, W_2, each about 1 in. square and separated by a black strip about 1 in. wide, thus allowing amply for the differences in inter-pupillary distances in adult subjects.

Inside the main apparatus BBBB (a box of hard-wood material, supported by a steel framework), and, as closely as possible to the windows, are two 3 in. × 3 in. half aluminised mirrors M_1, M_2, of ¼ in. plate glass. The aluminium had been sputtered on to the front surface of each to such a density as to give approximately 50 per cent. transmission and 50 per cent. reflection for white light when (as in Figure 1) the mirrors are at 45° to the line of vision. The mirrors are gripped at the base on small stands, with suitable screw adjustments for rotation, levelling or transverse movement; and these stands are fixed to the rigid steel framework of the stereoscope.

The optical principles should be clear from Figure 1. During the pre-exposure period both eyes look directly at a fixation mark F, at a distance of 24 in. When either of the cards C_1 or C_2 is illuminated this will produce an image at exactly the same optical distance from the eye concerned. The accuracy of the adjustment can be checked both by direct geometrical measurement of the apparatus and by simple monocular parallax tests. For convenience the cards used are ordinary 6 in. × 4 in. filing cards, upon which

any desired diagrams can be drawn, or pictures up to post-card size can be mounted. These cards are in holders which allow a fine adjustment backward or forward in the apparatus; i.e. to the left or right in the reflection seen in the subject's eye.

FIGURE 1



The normal setting is that the centre point of each card coincides optically with centre of the fixation mark, F. This mark is a St. George's cross finely drawn on an opaque lantern slide (each vertical or horizontal arm projecting ¼ in. from the centre). The luminosity can be varied over a very wide range by the insertion of suitable neutral filters (D in Figure 1) between the lamp L and the cross. Such a fixation mark, in conjunction with the mirrors M_1, M_2, has a very useful (but, admittedly, unexpected) feature. If we consider a ray of light from F through the mirror M_1 to the subject's right eye E_1, it is clear that double reflection in the glass of the mirror M_1 will produce a second, much fainter, image of F and that this will appear slightly displaced to the subject's left. Similarly the mirror M_2 will produce a "ghost" image slightly displaced to the subject's right. Thus if in the pre-exposure period the subject reports seeing clearly a figure of this form ⁞┼⁞ , we can be sure that he is correctly accommodated and converged, *and that both eyes are working.* The last point is illustrated rather dramatically by some very marked "eye-dominant" subjects who see only *one* vertical "ghost" line, and this—to their surprise, if they have not worked at the optics of the matter—is on the opposite side to what they may know to be their dominant eye.

The cards C_1 and C_2 are illuminated separately by lamps in compartments below the plane of Figure 1, and therefore not shown in the diagram. By trial and error adjustment of the position of the lamps, and of the size and position of the aperture connecting each lamp compartment with the corresponding card compartment it is easy to arrange a diffuse and sensibly uniform illumination of each card. That this is not a "brightness constancy" effect can be checked by photometric measurements of different parts of each card as seen in the mirror when the photometer is placed in the position E_1 or E_2. (An S.E.I. instrument is excellent for this purpose.) With the apparatus now in use, and with 40W bulbs, the apparent brightness of a plain white card is about 25 milli-lamberts.

The lamps are in a circuit (of which a diagram would be unnecessary) which allows that either can be switched on or off independently, or that both can be worked together. In addition, the provision of Venner time-switches in each circuit makes it possible to arrange that the times of onset and the duration of illumination of each card can be controlled automatically (to within limits of about ± 1 sec.).

The dimensions and details above refer only to the apparatus now in use at the Cambridge Psychological Laboratory: it is obvious that similar principles could be applied to other sizes of field, optical distances, and exposure times.

I should like to thank the work-shop staff of this laboratory for much help in construction, and also the staffs of the Clarendon Laboratory, Oxford, and The Cavendish Laboratory, Cambridge, for the preparation of the mirrors. I should also like to thank Dr. H. Asher, with whom I have had several discussions of binocular problems, and who, with a different form of apparatus has used half-reflecting mirrors (Asher, 1953) in their study.

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BOOK REVIEWS

Psychologie. Die Entwicklung ihrer Grundannahmen seit der Einführung des Experiments. By W. Metzger. Wissenschaftliche Forschungsberichte, Band 52, Darmstadt, Steinkopff, 1954. Pp. 407 + xix.

Gesetze des Sehens. By W. Metzger. Frankfurt am Main, Kramer, 1953. Pp. 470 + xii.

Professor Metzger's early work on visual perception is very well known in this country, largely through the fascinating account of it given by Koffka in his *Principles of Gestalt Psychology.* His new book—published last year—is an attempt to trace the main lines of development in theoretical psychology, with especial reference to the growth and findings of experiment. The approach is traditional in the sense of pre-war German scholarship. Among the topics dealt with (many of them at great length) may be mentioned the problem of sensory attributes and Gestalt qualities, the problem of spatial and temporal organization, the problem of figural dominance ("Zentrierung") and the body-mind problem. Although the lack of direct concern with problems of behaviour might lead some to dub Professor Metzger's work as "philosophical," it should be borne in mind that the growth of new pre-occupations in psychology does not necessarily imply the solution of old problems. To all who care about hard systematic psychological analysis, Professor Metzger's text may be recommended with pleasure and confidence.

Gesetze de Sehens is a revised and enlarged version of a book first published a few years before the war. It remains as one of the best and most vivid accounts of the Gestalt theory of perception, embellished by an enormous number of instructive and well-chosen illustrations—by no means all drawn from the laboratory. Among the new material included may be mentioned Köhler and Wallach's work on figural after-effects, Michotte's studies of phenomenal causation, and some account of experimentally induced spatial disorientations. Although not very critical, this book has great value in providing some most ingenious demonstrations and discussions of Gestalt principles and would abundantly repay translation.

Although German experimental psychology has far to go if it is to regain the lead which it held for so many years prior to 1933, Professor Metzger's work will certainly contribute to the re-establishment of traditional standards in European scholarship. One may also hope that it will divert some British psychological eyes from their present hypnotic fixation upon the Far West.

O. L. Z.

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APPARENT FLUCTUATIONS OF A SENSORY THRESHOLD

BY

R. C. OLDFIELD

From the Department of Psychology, University of Reading

An account is given of a method for the nearly continuous recording of sensory thresholds and other psycho-physical variables. Its application to the difference-threshold for sound-intensity is described. It is found that this threshold, so recorded, shows irregular fluctuations in time, excursions of up to 100 per cent. being commonly present. There is no *prima facie* indication of any regular periodicity in the fluctuations. No evidence was found of any general trend in amount of fluctuation throughout a half-hour period of observation. Nor, except in one subject, was there any tendency for the amount of fluctuation to increase or decrease over a series of six half-hour periods of observation. There are marked, and fairly consistent, differences between individual subjects as regards the amount and pattern of fluctuation. The possible locus and causes of these effects are briefly considered.

I

Introduction

The presuppositions and techniques of classical psycho-physics left little room for consideration of the possibility that thresholds might be quantities subject to inherent fluctuation in time. The notable variability of single threshold judgements, even in circumstances as uniform as the experimenter could devise, was handled by statistical means inspired by the theory of errors of observation in physical science. The conception of the threshold was largely that of a definite quantity to be disentangled from the "errors" inevitably introduced in the process of determining it. Inherent fluctuations, if they were present, were thereby smoothed out along with truly random variations. The effect of this expedient was not lessened by the time-consuming procedures demanded by statistical design. Full application of the constant method, for example, might well demand a set of observations lasting over a period of an hour, with one threshold-value as the outcome. But if fluctuations are present, they may quite well be supposed to occupy periods of the order of minutes or even seconds.

From another point of view, however, interest in the possibility of fluctuations in sensory capacity arose from the observations of Urbanschitsch (1875), and was considerably debated during the subsequent fifty years under the heading of "fluctuations of attention." Urbanschitsch reported that weak stimuli (around absolute threshold level) of constant intensity were reported by observers as subject to irregular appearances and disappearances. If a number of observers simultaneously listened to the ticking of a watch, for instance, all would report periods during which it was audible, interspersed with periods during which it was not. Other faint auditory stimuli gave the same result. Urbanschitsch himself, having as he thought exonerated

the outer and middle ear by the use of several subjects with perforated ear-drums, and one with a disarticulated stapes, attributed the phenomenon to fluctuations in the "perceptive ability" of the auditory nerve, amounting to successive periods of fatigue and recovery. Lange (1888) pointed to a number of objections to this view, and, believing he had found simultaneous fluctuations in the auditory and visual modes, was led to think that fluctuations of attention were involved. The subsequent controversy, fully and clearly summarized by Guilford (1927), explored a number of possibilities, among them the role of peripheral adjustive mechanisms, such as eye-movements and pupillary and lens accommodation, relationship to the Hering-Breuer waves, and central factors other than attention. The conclusions were uncertain, but Guilford (1927) successfully demonstrated that the statistical distribution of the phases of appearance and disappearance of a constant (visual) stimulus near threshold-value is the same as that of the positive judgements in a psycho-physical experiment, and follows the *phi-gamma* hypothesis. He concluded that the assumption of fluctuations of attention is superfluous.

To the question of fluctuation of threshold in its relationship to attention, the findings of Head (1920) upon subjects with parietal lobe lesions are not without relevance. Head noted, among patients in whom cortical lesions had produced disturbance, but not loss, of cutaneous sensibility, a very marked variability and unreliability of sensory judgement. Sensory responses were not graded in relation to the intensity of the stimulus. The patient behaved, so far as the affected body-area was concerned, like a normal person in an extremely inattentive state, and Head spoke of a "loss of local attention." He regarded such disturbances as due to a destruction or impairment of the physiological (cortical) mechanisms by which stimuli are normally brought into appropriate relationship to one another, and thus lead to consistent conscious judgement.

Godfrey Thomson (1914) put forward definite evidence of fluctuation in the two-point threshold during a single session. Using the methods of non-consecutive serial groups and of right and wrong cases, but treating the data by the limiting method, he found changes occurring over ten determinations of the threshold, extending over approximately ten minutes. A number of the differences amounted to six times the probable error. The trend revealed by averaging results for four subjects for 65 sessions (after correcting for diurnal variations and inter-subject differences) was a fall in threshold over the first six minutes, a rise around the seventh minute, followed by a fall to the tenth. Other workers have since investigated the variation within a session, in several sensory modes. Blackwell (1953) for instance measured the difference-threshold for light intensity at various times after the beginning of a session of eighty minutes length, and found effectively no evidence of variation when a "temporal forced choice" method of report was used, and very slight trend upwards towards the end of the session when a "yes-no" form of judgement was adopted. Like Thomson, however, Blackwell averaged the observations of a number of sessions, and his conclusions can only relate to the absence of any *fixed* temporal pattern of threshold change throughout the session which remains the same in a number of different sessions. The presence of aperiodic fluctuations within each session, or of periodic oscillations whose period varied from session to session, would not, of course, be revealed in his results.

The closest study of fluctuations within a single session is that made by Wertheimer (1951, 1953), who obtained yes-or-no judgements to a single stimulus intensity, alternately for light and sound near threshold, at six-second intervals throughout sessions lasting fifty minutes. The results were analysed by a number of methods, including autocorrelation and power-spectrum analysis. Statistically significant

periodicities were found, with some correlation between the two sensory modes. The actual amount of threshold-fluctuation over the six-second interval occurring in Wertheimer's experiments cannot be assessed, since his data consist of percentages of positive responses to a single-stimulus value. Further experiments, using longer time-intervals between judgements, gave similar results, with periodicities up to 27 minutes. Wertheimer also examined possible day-to-day variations, and considered he had found evidence of significant changes in his observations. Other workers, e.g. Hopkinson (1952) and Harper (1952), have likewise noted such longer term variations.

It would appear, then, that there is some *prima facie* evidence for fluctuation and other forms of change or instability in at least some sensory thresholds.

The present communication reports observations on fluctuations of the difference-threshold for sound-intensity, measured by one particular method in one set of conditions. Specifically, answers were sought to the following questions:—

(1) To what extent does the threshold vary during a period of half an hour?

(2) What are the general characteristics of such variation and, in particular, is there obvious evidence of either any well-marked periodicity, or of any general trend?

(3) Are there differences in the amount of fluctuation at different times within the period of half an hour?

(4) In a series of half hour sessions, is there evidence of any systematic change in the nature and amount of fluctuation?

The data obtained allow us to give answers to these questions. These answers might not, of course, apply if the conditions of the experiment were changed. It is hoped to report in a later communication further experiments bearing upon the factors determining the effects observed.

II

Method

General Principle.

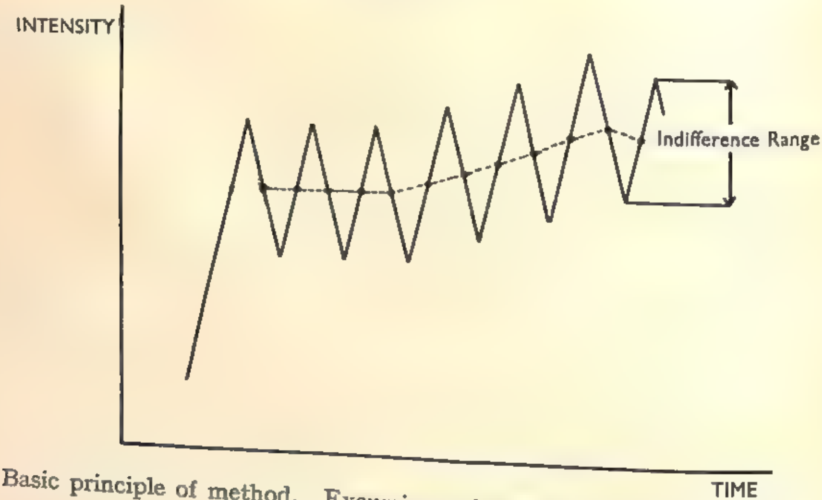
A brief account of the method used in this investigation has already been published (Oldfield 1949). The general principle involved is perhaps most easily explained by reference to its use for the determination of an absolute threshold, although it is in fact a difference-threshold with which we are concerned in this paper.

Suppose that we wish to determine the absolute threshold for sound intensity. The intensity of a tone, supposed initially to lie below the threshold, is increased at a constant rate by a mechanical device. The subject is given a push-button which he is instructed to press when he hears the sound, and to keep pressed for so long as he hears it, releasing it when, and for so long as, he fails to hear anything. When the button is depressed, the effect is immediately to reverse the mechanical control of the stimulus-intensity, so that the latter now diminishes at the rate at which it previously increased. The subject ceases to hear the tone, and releases the button. The control again reverses, and the cycle is repeated. Movements of the control are mechanically recorded on moving paper, and consist of a series of excursions of the type shown in Figure 1. We shall refer to the range of stimulus-value over which an excursion is made as the *indifference-range*. A change in the threshold-value will bring about a shift in the pattern of excursions, and the stimulus-values corresponding to the mid-points of the excursions will be referred to as values of the *cursive threshold*. This arbitrary term is introduced in order to distinguish the threshold so determined from measures resulting from other procedures, and to avoid gratuitous presupposition

as the nature and causes of any spontaneous fluctuations which may be found in its value.

It will be seen that the process involved is essentially a cyclical one incorporating negative-feedback, the subject acting as a discontinuous control-element in the loop. Bekesy (1947) seems to have been the first to use this principle, which he applied to audiometry. (Ignorance of this prevented acknowledgement when my first account was published.)

FIGURE 1



Basic principle of method. Excursions of control shown in heavy line, connected mid-points of excursion in dotted.

The method was originally devised with a view to the study, in terms of a sensory indicator, of central conditions such as fatigue, adaptation and "vigilance." For this purpose a process was needed in which recording of the threshold would be as continuous as possible, in which the subject might be regarded as being in a steady state, and performing a simple standard task. I also wished to have a principle capable of application to a wide variety of sensory and perceptual situations. The method described appeared to satisfy these requirements, but preliminary experiments soon made it plain that the underlying process is by no means so simple as it might seem at first sight. Fluctuations in the cursive threshold, of considerable magnitude and seemingly spontaneous character, made their appearance in the records, and it became clearly essential to study their nature and conditions before proceeding to the applications of the method which were originally in mind.

Another aspect of the method should be emphasized. Regarded in one light it appears as a psychophysical experiment, differing from other experiments of this kind in that each of the subject's decisions gives rise automatically to the conditions which demand the next. But the subject's task may, perhaps, be equally well regarded as the exercise of a very simple adjustive skill, in which he endeavours by means of an all-or-nothing motor response to maintain a certain required state of affairs, namely that the stimulus-magnitude should remain at or around the threshold level. This consideration suggests that the results may need interpretation in a broader context than the purely psychophysical, and might even make some contribution to a theory of skills as well as to an understanding of the central aspects of sensory processes.

As regards the cycle of operations itself, its quantitative characteristics, and in particular its duration, can be seen to depend upon a number of variables. The

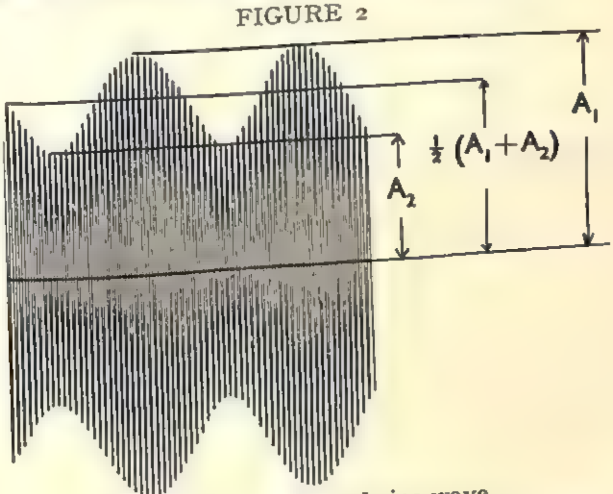
difference of stimulus-magnitude between the ascending and descending thresholds, the rate of change of stimulus-magnitude imposed by the mechanical control, and the subject's reaction time all play a part. More subtle factors, such as the criteria of judgement adopted by the subject, and his tendencies to anticipate change, and to fall into a temporal rhythm of pressing and releasing the button, also play a part. Of these the only factor under the effective control of the experimenter is the rate of change of stimulus-magnitude. In practice it was found convenient to adjust this to give one complete cycle of operations in about five seconds. This incorporates, of course, *two* threshold decisions, one ascending and one descending.

The present experiment.

The present experiment is, however, one upon the *difference*-threshold for sound intensity. This threshold was chosen to allow the experiments to be conducted at a considerably supraliminal level and thus off-set the lack of a fully sound-proof room. The form of stimulus-presentation was that used by Riesz (1928). The subject listens to a pure tone which is amplitude-modulated by a sine-wave having a frequency of the order of seven per second. When the modulation-depth is below a certain value he perceives a pure tone of unchanging loudness. As the modulation-depth is increased he is aware of a tone which fluctuates in loudness at the frequency of the modulating wave. In our experiment it is the modulation-depth which is automatically increased and decreased linearly with time, and the threshold is accordingly recorded as percentage modulation.

It should be noted that the threshold-values so obtained are therefore different from corresponding values stated, as is more common, in terms of *intensity*, though connected by a simple relation. In the present experiment it is arranged that the modulation takes place in such a way that the intensity averaged over one modulation-cycle remains constant, irrespective of modulation depth, as shown in Figure 2.

FIGURE 2



Amplitude modulated sine-wave.

If, then, A_1 is the maximum amplitude, and A_2 the minimum, the quantity $T_c = 2(A_1 - A_2)/(A_1 + A_2)$ is linearly represented on our records, and may conveniently be taken as an index of differential sensitivity. The more conventional $\Delta I/I$ will be equal to $(A_1^2 - A_2^2)/A^2$, where A may be taken to be A_1, A_2 or $\frac{1}{2}(A_1 + A_2)$ according to arbitrary choice, with little difference if $(A_1 - A_2)$ is small. Adopting the last alternative as most convenient, we have for the intensity threshold

$$\begin{aligned}\Delta I/I &= 4(A_1 - A_2^2)/(A_1 + A_2)^2 \\ &= 4(A_1 - A_2)/(A_1 + A_2) \\ &= 2\,T_c\end{aligned}$$

The threshold as indicated on our record is, therefore, one half of the threshold expressed as $\Delta I/I$.

A further point of some importance is the following. We have said that the stimulus consists of a pure tone sinusoidally modulated in amplitude. This may be expressed analytically in the form

$$\xi = \tfrac{1}{2}(A_1 + A_2) \sin 2\pi (F + f)\, t + (A_1 - A_2) \cos 2\pi ft \sin 2\pi Ft$$

where A_1 and A_2 have the same meaning as before, F is the modulated, and f the modulating, frequency.

But this is identically equal to

$$\xi = A_1 \sin 2\pi (F + f)t + \tfrac{1}{2}(A_1 - A_2) \sin 2\pi (F - f)t,$$

and it is clear that the stimulus may equally well be regarded as a mixture of two pure tones beating together. (In Riesz' experiment, though not in ours, this was in fact the way in which the modulated tone was produced.) The subject's decision might, therefore, be determined, not by the perception of a fluctuation in loudness, but by his noticing the intrusion of a second tone upon a steady note. In practice it seemed, from the accounts given by the subjects, that at times the stimulus appeared to them as a pure tone of fluctuating loudness, and at others as a pure steady tone to which a second tone was added. As one of them expressed it, it was like the reversal of figure and ground in an ambiguous design.

The actual conditions in which the present experiment was performed were as follows. The pure tone was one of 500 c.p.s., this frequency being chosen in preference to a higher one on account of its greater capacity to mask extraneous noises. The intensity of this pure tone was approximately 65 Db above its own threshold measured at zero modulation. It was modulated at a frequency of 7·4 c.p.s. Riesz found that differential sensitivity was at a maximum for a modulation of 4 c.p.s., the threshold rising by about 25 per cent. at 7·5 cycles. The present figure was chosen as it was convenient from the point of view of the recording apparatus that the threshold values should not be too low, while at the same time the subjects found their judgements easier to make than with lower modulating frequencies. The stimulus was applied to both ears. During the experiments the subjects were seated alone in a small "sound-proof" cabinet, 4 ft. by 4 ft. 6 in. by 6 ft. This was windowless, and provided with double-doors. It was ventilated by a forced draught system, the ducts of which contained sound-absorbent splitters. Fan noise was inaudible against the stimulus tone. The amount of attenuation provided by the cabinet itself was not high, but, supplemented by the wearing of close-fitting earphones and the masking effect of the stimulus-tone itself, was sufficient to render inaudible the noises of the control and recording-apparatus, situated outside the cabinet, and other extraneous sounds. The temperature in the cabinet was, unavoidably, somewhat high—around 25°C—but stable from day to day. Humidity was low, and stable at approximately 30 per cent.

III

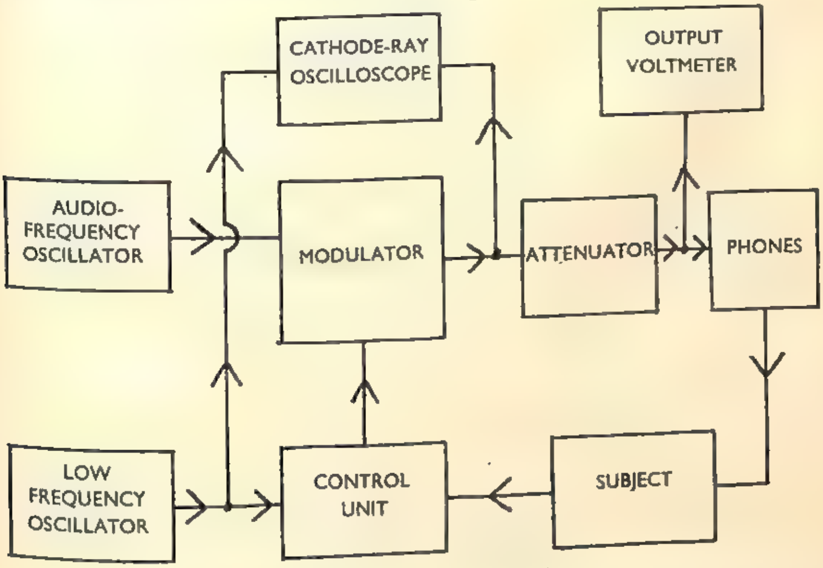
Apparatus

A block-diagram of the apparatus is shown in Figure 3. The output from a Muirhead Resistance Capacity oscillator at 500 c.p.s., about 0·25 volts r.m.s., is fed into a modulator of special design. The output from a low-frequency R.C. oscillator at 7·4 cycles and about 1 volt max. is fed to a potentiometer situated in the Control Unit. The stepped-down voltage from this potentiometer passes to the modulator. The modulated output from the latter is fed *via* a matching-transformer to the 600 ohm input of the attenuator, the output of which is matched by another transformer to a pair of Brown Type K moving-coil headphones.

The Control Unit consists of a small low voltage D.C. motor with gear-chain, enclosed, together with its reversing-relay, in a box for silence. The final shaft speed was 6·7 r.p.m.

Reversing was carried out by commutation of the armature supply by means of a double change-over relay, operated by the subject's press button. The latter was an ordinary pear-shaped bell-push, chosen for its lightness of operation. The motor-shaft was connected to the spindle of the potentiometer by a drive which, though flexible, introduced no appreciable backlash. A ballast resistance was introduced into the motor supply circuit, to moderate the violence of the reversing action, but, as judged by the recorded tracing, the reversal was sensibly instantaneous.

FIGURE 3



Block diagram of apparatus.

Also mounted on the motor shaft was a pulley whose circumference was equal to the range of travel of the recording-pen. A length of good fishing-line was attached to the pulley and wound twice round it, tied to the pen and passed on over a second pulley to a counterpoise. Safety-stops in the form of pairs of contacts prevented the pen and control gear from travelling beyond their limits, and the circuit was so arranged that the subject's push-button would over-ride their effect. Thus if, as happened at times, the subject allowed the control to proceed to the maximum modulation condition without pressing his button, the control would make small oscillations to and fro at this position, and be ready to respond to the push-button when it was next pressed.

Monitoring and Calibration.

The modulating oscillator output was checked visually against a linear time base on a D.C. oscilloscope. This voltage was then used as a time-base against which to display the output to the telephones. The picture thus obtained has the outline of a trapezium, the shorter of whose two vertical sides represents the minimum amplitude of the modulated 500 c.p.s. wave, and the longer the maximum. Measurement of these sides for various positions of the control potentiometer and pen thus allowed the apparatus to be calibrated. At the same time, any departure from linearity on the part of the modulator is easily detected, since the two inclined sides of the trapezium then cease to be straight. Under all conditions used in the experiment, the behaviour of the modulator was found to be sensibly linear.

The actual output to the telephones was checked by measurement with a valve voltmeter, and maintained constant throughout the experiment.

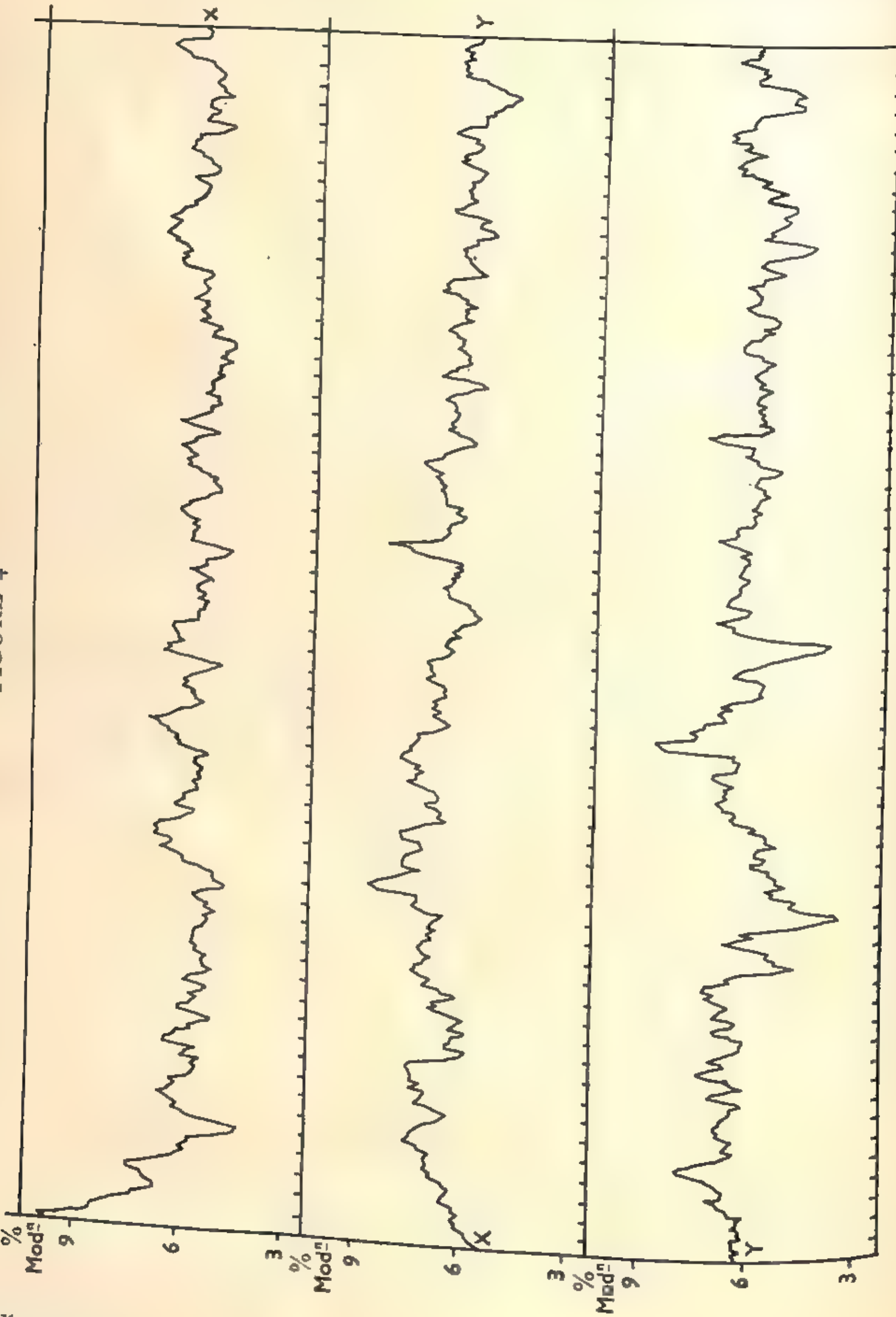
IV

Procedure

Five adult subjects, two male and three female, were used. All were found to be otologically normal by the E.N.T. Department of the Royal Berkshire Hospital, with

the exception of one man who had a localized loss in both ears of some 25 Db. at 8192 cycles to air-conduction. This was thought immaterial, as the highest frequency involved in the experiment is 507·5 c.p.s. None of the subjects had any history of

FIGURE 4



Tracing of complete record of 30-minute session. Time-marks = 10 secs.

middle-ear disease. The subjects were all members of the research and secretarial staff of the Department of Psychology, Reading. Their ages ranged from 22 to 34.

Each subject attended on seven occasions. At the first, a practice session, the results of which were discarded, the nature of the experiment was explained and the changes in the modulated tone demonstrated on a loudspeaker, together with the effect of the push-button. The subject was asked to try to settle down for the period of the experiment (of whose length he was told) and simply press the button and keep it pressed for as long as the tone appeared to fluctuate in loudness, releasing it when the tone appeared steady. He was warned against adopting over-precise criteria, and told to try to maintain steady over-all behaviour. The sound-proof cabinet was then closed, the stimulus switched on at full modulation, and the control unit switched to automatic. This first, practice, session lasted for a quarter of an hour. During this time the maximum modulation level was adjusted so as to place the subject's record conveniently on the paper. This level was generally found suitable on subsequent occasions.

The six experimental sessions were similar, except that each lasted for half an hour. It was unfortunately impossible to arrange these further occasions at regular intervals, but the whole series was completed within a period of six weeks, and no subject had more than one session in a single day.

V

Results

Our data consist of thirty records each incorporating an irregular curve formed by joining the successive mid-points of the excursions of the stimulus control. The not inconsiderable labour of plotting the mid-points may be alleviated by the use of a simple geometrical gadget. (A form of recording apparatus which automatically plots the mid-points, and not the excursions, has been constructed. It is hoped to describe this in a forthcoming communication.) The difficulties and pitfalls which attend any attempt to obtain a complete analysis of periodicities and trends in such curves are notorious, and no elaborate statistical treatment is contemplated here. But in order to gain some quantitative notion of the effects found, the following preliminary reduction of the data was carried out. Nine periods each of three minutes were selected in each record. These were, in general, the first nine periods—it was not always found possible to select ten full three-minute periods. A tracing of horizontal lines corresponding to intervals of $\Delta I/I$ of 1 per cent. was placed over the record, and the proportion of each period during which the curve lay above each of these lines was measured. These values were plotted against $\Delta I/I$, giving cumulative distribution curves. On each of these the median value, and the semi-interquartile range were read off.

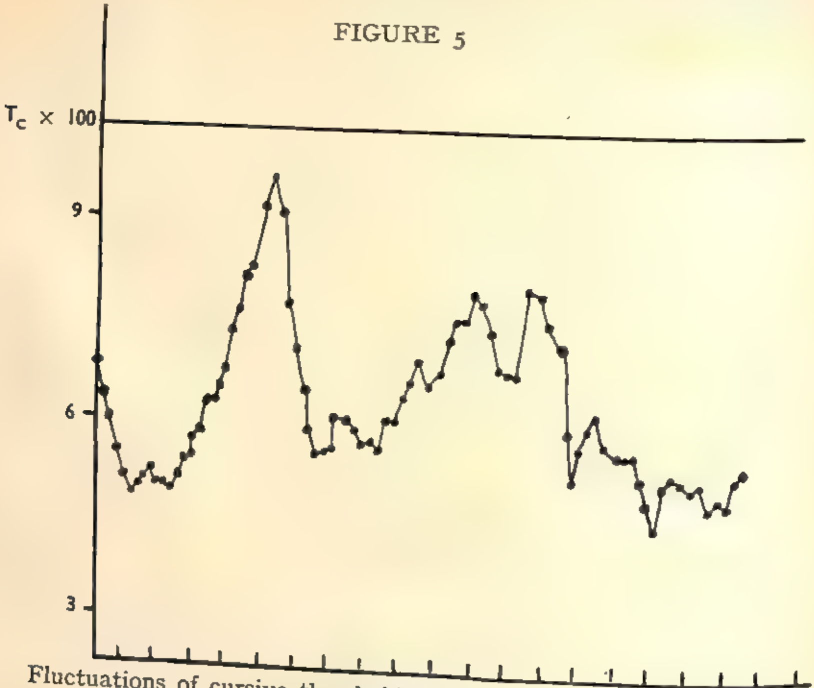
General Character of the Records.

The whole of one record is reproduced on a reduced scale in Figure 4, and a typical three-minute period from another is shown in Figure 5. General inspection of the whole series reveals the following characteristics:—

(1) There is no general trend up or down of the cursive threshold itself throughout each half-hour period. Since no special precautions were taken to ensure complete constancy of calibration as between *different* sessions, the absolute values of the cursive threshold obtained are not comparable—only the amounts of fluctuation.

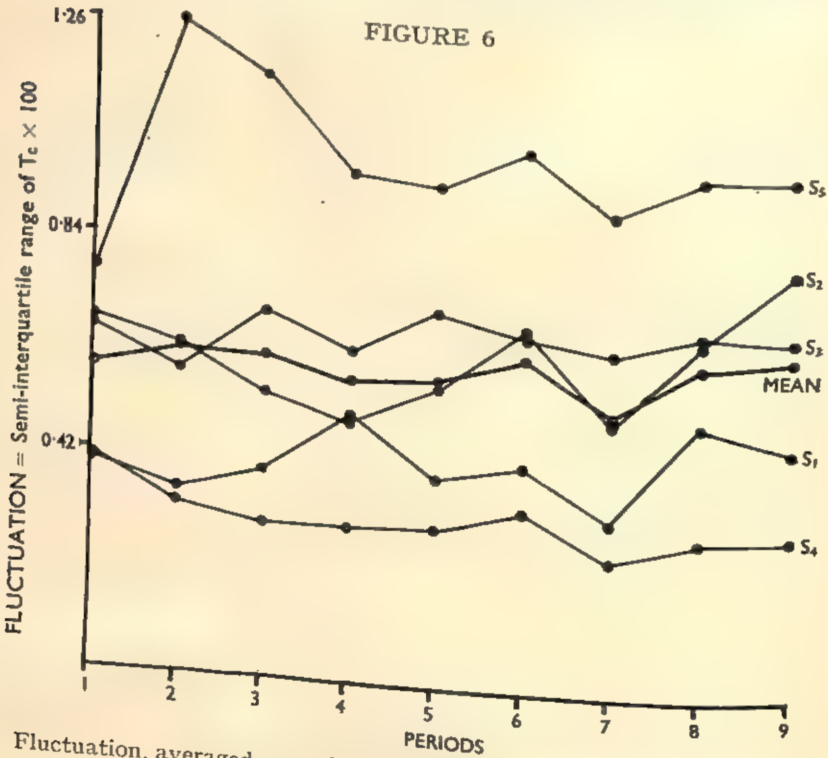
(2) All records show marked fluctuation, variations of 100 per cent. in threshold value occurring over periods of the order of a minute being by no means uncommon.

FIGURE 5



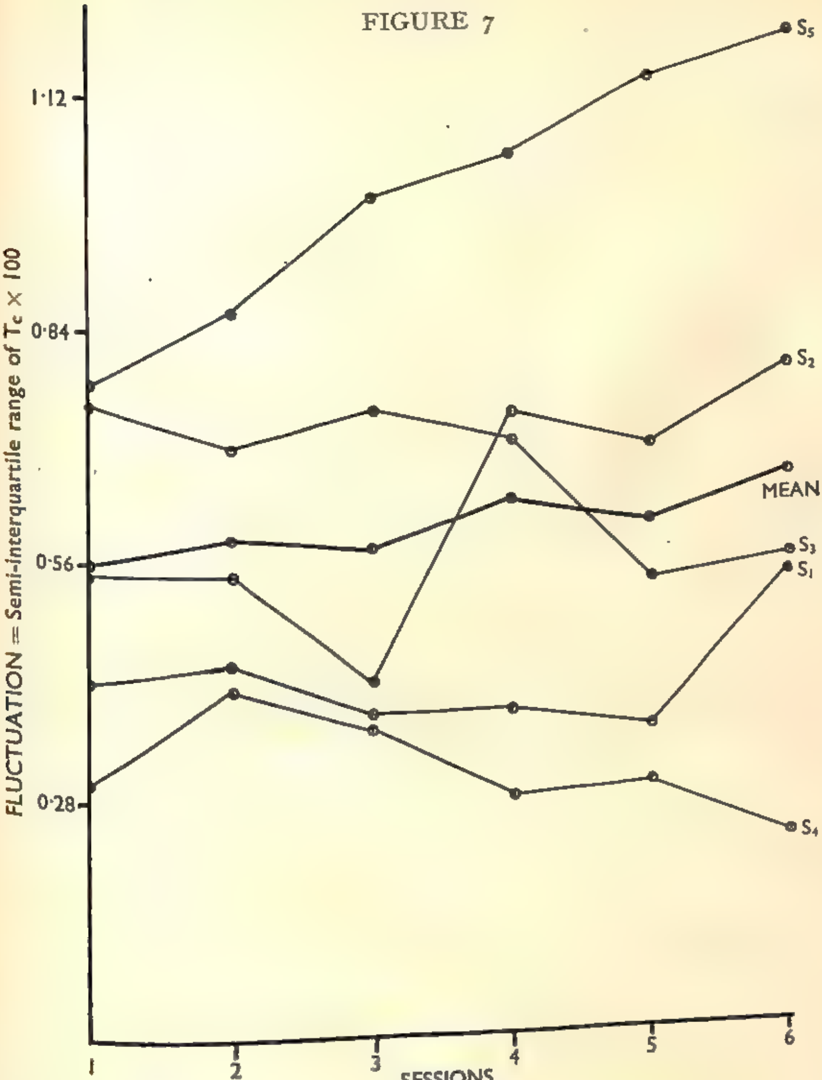
Fluctuations of cursive threshold (T_c) during one 3-minute period. Dots show successive threshold judgements. Time-marks = 10 secs.

FIGURE 6



Fluctuation, averaged over all sessions, by periods for separate subjects, and mean.

FIGURE 7



Fluctuation, averaged over all periods, by sessions, for separate subjects, and mean.

(3) No general trend in threshold value is apparent throughout each 30-minute session.

(4) While a number of the fluctuations have a fairly simple oscillatory character, no clear periodicity reveals itself to the unassisted eye.

(5) The amount of fluctuation appears to be fairly constant throughout each session, and there is no marked trend in amount of fluctuation from session to session, except in the case of subject 5, whose fluctuation rose steadily throughout the series.

(6) There are notable differences between different subjects. These concern (*a*) the size of the indifference-range, (*b*) the constancy or variability of the indifference-range, (*c*) the amount and general character of the fluctuation. So characteristic were these features that records by different subjects could often be distinguished and even identified at a glance.

(7) The great majority of the records show an exponential-form drop in threshold extending over the first minute or so. This would correspond with what is usually taken as a "practice" period in a conventional psychophysical experiment. It amounts to a fall of about a 100 per cent. of the threshold value, and where it appears is so regular in form as to suggest some specific adaptation process rather than a general habituation to the experimental situation. Its presence was no less marked in the later sessions than in the earlier.

Quantitative Analysis.

These general findings are borne out by such quantitative analysis of the data as it is possible to make. Figure 6 shows the variations in the amount of fluctuation by periods, the values plotted being averages taken over all sessions. Similarly Figure 7 shows the variations by sessions, the averages being taken over all periods. In the former case the mean of all subjects, represented by the broken line, shows substantial constancy, while in the latter there appears a small but progressive rise in mean amount of fluctuation throughout the sequence of sessions, although this is largely contributed by subject 5, whose apparently anomalous behaviour can be seen from her individual graph.

As would be expected, the population of values of semi-interquartile range is highly skewed, and neither logarithmic nor square-root transformation goes far to normalize it. In view of this, any attempt to separate out subject, period and session effects by analysis of variance would be inappropriate. Instead a simple treatment by m-rankings was adopted. We may examine first the question of individual differences. Suppose that for each *session*, we arrange the subjects in order of the amount of fluctuation shown within that session. Then if there are consistent individual differences between the subjects prevailing throughout the sessions, the order should be to some extent concordant. This may be tested by calculating the coefficient of concordance, W. (Kendall 1948). (In each case, the lower the rank number, the smaller the amount of fluctuation.)

We have:—

		Sessions (m) 1	2	3	4	5	6	*Total Ranks*	*Over all Ranks*	
Subjects (n)	1	2	2	2	2	2	2	12	2	*Ranks*
	2	3	3	3	4	4	4	21	3½	
	3	4	4	4	3	3	3	21	3½	
	4	1	1	1	1	1	1	6	1	
	5	5	5	5	5	5	5	30	5	

In this case tables for the exact treatment are available (Kendall 1948, App. 6). We find $S = 344$ and $P << 0{\cdot}01$. We conclude that there are marked individual differences. The same conclusion emerges if we consider the subjects' ranks in respect of mean fluctuation in each period, the mean being taken over all sessions. In this case we have $S = 728$, $P << 0{\cdot}01$.

Secondly, we may regard the subjects as five different "judges" who each rank the *periods* in order of amount of fluctuation occurring in each. If there is some tendency for fluctuation to be greater in some periods than others, there will be some agreement between the judges as to their rank order. We therefore average the amount of

fluctuation for each period over all sessions and, for each subject, rank the periods. We obtain the following table:—

		Subjects (m) 1	2	3	4	5	Total Ranks	Over all Ranks	Ranks
Periods (n)	1	5	6½	4	9	1	25½	6	
	2	2	5	1	7	9	24	3	
	3	3½	3	8	2½	8	25	5	
	4	8	1	2	2½	4	17½	2	
	5	3½	4	9	5	3	24½	4	
	6	6	8	5	8	7	34	9	
	7	1	2	3	1	2	9	1	
	8	9	6½	7	5	5½	33	8	
	9	7	9	6	5	5½	32½	7	

In this case, tables for the exact treatment are not available. We therefore calculate W and carry out z-transformation. We find $W = 0{\cdot}344$, $z = 0{\cdot}370$, $P > 0{\cdot}05$, and there is no evidence of a pattern of fluctuation throughout the periods. The curious concordance between the subjects as to the small amounts of fluctuation in the seventh period is probably to be regarded as fortuitous, since it does not form part of any more general and widespread trend revealed in the total ranks of the periods. It should be remarked, in addition, that so far as each individual subject is concerned, the high rank for the seventh period is established only by a narrow margin of score.

Finally, we may rank *sessions* in respect of amount of fluctuation shown in them, with a view to finding whether there is any common pattern of change. We have:—

		Subjects (m) 1	2	3	4	5	Total Ranks	Over all Ranks	Ranks
Sessions (n)	1	4	2½	6	4	1	17½	3	
	2	5	2½	3	6	2	18½	5	
	3	2	1	5	5	3	16	2	
	4	3	5	4	2	4	18	4	
	5	1	4	1	3	5	14	1	
	6	6	6	2	1	6	21	6	

$S = 28{\cdot}00 \qquad P >> 0{\cdot}05$

We conclude that there is no evidence of any consistent pattern in the amount of fluctuation throughout the sequence of sessions.

VI

Discussion

Pending completion of further experiments now in progress, it would be premature to speculate as to the causes of the fluctuations we have observed. It may, however, be worth considering briefly the question of whether the locus of the effects is likely to be found in the peripheral sense-organ, or more centrally.

It might, perhaps, be suggested that conditions in the ear itself, produced for example by continued pure-tone stimulation, or by the continued wearing of head-phones, might occasion changes in its functions which could manifest themselves in

an instability of threshold. It is certainly true that the experience of listening to a pure tone of constant intensity at this level is marked, subjectively, by irregular waxing and waning of its loudness. Whether such an effect should be related to the variable operation of vaso-motor reflexes, or of stapedius and tensor tympani activity, or to some other, possibly central, mechanism, there is at present no evidence to show. But it would seem doubtful whether any hypothesis of this character could in itself bear upon fluctuations of the *difference*-threshold. At the level of operation in question, the Δ I/I — I curve is flat, and considerable changes in the response-level of the ear ought to leave the difference-threshold unchanged. Again the wearing of headphones for periods of the kind demanded by our experiment not uncommonly leads to a feeling of congestion in the ear. This is sometimes relieved by swallowing, with concomitant change in the perceived sound-level, suggesting that pressure differences may build up across the tympanic membrane, with resulting shift in the operating level. But to this suggestion the same objection would hold as before.

Lastly, the question of "fatigue" or adaptation might be supposed to arise. Rawdon-Smith and Sturdy (1939) showed that if the ear be exposed for a period of two minutes to a tone (1000 c.p.s.) 70 db. above threshold, the *difference*-threshold is in general altered. The change is at a minimum when measured at the same intensity-level as that used in producing the change, and amounts to some 15 per cent. Rawdon-Smith and Sturdy's experiment was not designed to afford particularly accurate estimates of the amount of change, but it may be noted that we observed *fluctuations* of the threshold much larger than the *change* reported by them. It should further be remarked that their experiment was carried out at 1000 c.p.s., and Rawdon-Smith (1936) had already shown that, so far as the production of changes in *absolute* threshold by adaptation is concerned, effects produced at 500 c.p.s. are negligible by comparison with those at 1000 c.p.s. and over. Rawdon-Smith and Sturdy, however, comment upon the markedly *labile* character of the effects observed by them, and this together with other indirect evidence, suggested to them that the effects are central rather than peripheral in origin. We may perhaps conclude that the fluctuations observed in our own experiments might have been produced by the 65 db. pure tone employed, *via* some adaptatory mechanism akin to that responsible for the effects observed by Rawdon-Smith and Sturdy. If so, this would support a central as opposed to a peripheral locus for the fluctuation.

In summary, we may say that there is no property of the ear itself, at present definitely known, which would be likely to lead to the threshold-fluctuation we have observed. The lability of the phenomena, and the marked individual differences, would suggest on the other hand a more central locus.

I wish to thank most gratefully the Medical Research Council for a grant for the purchase of apparatus, Dr. Schuster who made the recording gear, the Electronics Section of the Physics Department of this University for constructing the special oscillator and modulator to my requirements, Mr. Hunt Williams, F.R.C.S., for arranging for the subjects to be otologically examined, Mr. J. W. Whitfield and Mr. A. R. Jonckheere for statistical assistance, Miss E. M. Horrox for carrying out the tedious process of plotting the records and checking the statistics, and last but not least the subjects for their patience.

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CENTRAL INHIBITION—SOME REFRACTORY OBSERVATIONS

BY

ALICK ELITHORN AND CATHERINE LAWRENCE

From the Neurological Research Unit of the Medical Research Council, National Hospital, Queen Square, London

The observation that the response to the second of two stimuli is delayed if a response has to be made to the first has led to the development of a theory of a central refractory state during which incoming stimuli cannot be elaborated. In the experiment reported here the two stimulus-response situations have been made as independent as possible, and under these conditions it is shown that this theory cannot be maintained in its present form. The concept that the central integráting mechanism readily becomes blocked by nay single stimulus is dismissed as nonproven and uneconomic. The present findings, however, confirm previous observations that some interference between the two situations may occur but indicate that this is not necessarily maximal immediately after the presentation of the first stimulus. It is tentatively suggested that the phenomena of central inhibition can be interpreted in terms of the interaction between the excitatory and inhibitory significance of the stimuli and the internal anticipatory set.

Introduction

Much controversy in the early literature on the psychology of skilled movements centred round the discussion as to whether attention could be divided between two skilled tasks (Woodworth 1938). Most authorities favoured the view that such a division of attention was in fact impossible. It was held that where simultaneity of performance was apparent this was achieved by a rapid alternation of attention between the two tasks. Ryle, who is not addicted to dichotomies—in this case "all tasks must be either attended to or not attended to"—has pointed out that this difficulty may be largely a verbal one. He believes that the concept of heed or attention belongs to that class of dispositional statements which is both categorical and hypothetical (Ryle 1949). In this connection it might be pointed out that if skill is regarded as a function of attention then no activity could be regarded as fully skilled unless it received full attention. Further if two skilled tasks are undertaken simultaneously then one of them cannot be more than half skilled, for at least an equal amount of skill or attention must be left for the other. Perhaps it is not very meaningful to believe that when a juggler becomes skilled at one performance and can start to add to it another, then the first performance becomes reflex or unskilled.

Recently some observations on the limits of human ability in tracking situations (Vince 1948) and in the performance of serial reactions (Hick 1948; Vince 1948) have lead to the development of the concept of the Psychological Refractory Period and to the elaboration of a theory with wide implications. This theory which holds essentially that the integrative nervous activity of the organism is steplike and seriatim rather than diffuse and continuous has been proposed formally by Welford (1952) who states succinctly that "No two central organizing times can overlap." It is true that his immediate context refers only to two stimuli requiring hand action but the principle is advanced as a general one. Clearly there can be no doubt that several "reflex" integrative activities can co-exist and the concept of the "psychological refractory period" is a lineal descendant of the previously mentioned classical contention that

attention or consciousness is an indivisible attribute of the organism which at any instant of time can only characterize one of its activities.

Welford in his stimulating discussion states that the evidence would seem clearly to indicate that this refractoriness is in the central mechanisms themselves. He puts forward the hypothesis that this refractoriness is due to the central processes concerned with two separate stimuli not being able to co-exist, so that the data from a stimulus which arrives while the central mechanisms are dealing with data from a previous stimulus have to be "held in store" until the mechanisms have been cleared. It is this hypothesis that we decided to test. Welford himself says that the phenomenon of the psychological refractory period does not depend on the use of the same muscle groups and that the theory is specifically applied to central processes. It was decided therefore to make the two stimulus-response situations as independent as possible.

Methods

The subject was seated comfortably at a table with the fingers of each hand resting on a reaction time key. Facing him and 1·5 meters distant was a black bakelite board whereon was mounted a display consisting of a central fixation point which could be illuminated from behind and on each side of which there was at a distance of between 10 cm. and 15 cm. a cluster of five neon indicator bulbs. He was told that when the right-hand neons lit up he was to press the right hand key and to press the left hand key when the left-hand neons lit up. He was further told that he would be given approximately three seconds warning "Now" that on hearing this he was to look at the fixation point which would be illuminated for half a second before the first set of neons lit up and that the remaining set of neons would light up at a variable interval after the first. Each set of neons remained alight for approximately 10 m.s.

The time separation between the two stimuli was either 0, 20, 50, 100, 250, 370, 500, 600, 750 or 950 m.s. and this interval was varied randomly as was the side on which the first stimulus appeared. For two subjects the randomization was restricted so that the side of the initial stimulus was never the same on more than four consecutive occasions nor was the time interval the same twice running. For the third subject who knew these restrictions, randomization was complete. To derive these random series the tables of Fisher and Yates (1948) were used.

Since it was the second situation which was being studied, and since the variation of the time interval between this situation and the first determined a similar variation in the time at which the second situation was presented in relation to the warning signal, it was thought necessary to ensure that variations in the reaction time of the second response were not due to variations in the length of the period between the warning signal and the presentation of the second stimulus. During control periods therefore the first stimulus—at time 0—was omitted and this omission the subject was warned to expect. With one subject further control observations were made. The first stimulus was again omitted and the subject was told on which side but not at which time interval to expect the second stimulus. It will be seen that sometimes during the main experiment the so-called second or critical stimulus was presented simultaneously with the so-called first stimulus. Similarly we have said that the second stimulus was presented alone during the control periods. In order therefore to avoid confusion the situation whose time of presentation was varied will be referred to as the critical stimulus-response situation or S_c in contradistinction to the interference stimulus response situation S_1 which was presented at time 0, except during the control observations when it was not presented at all.

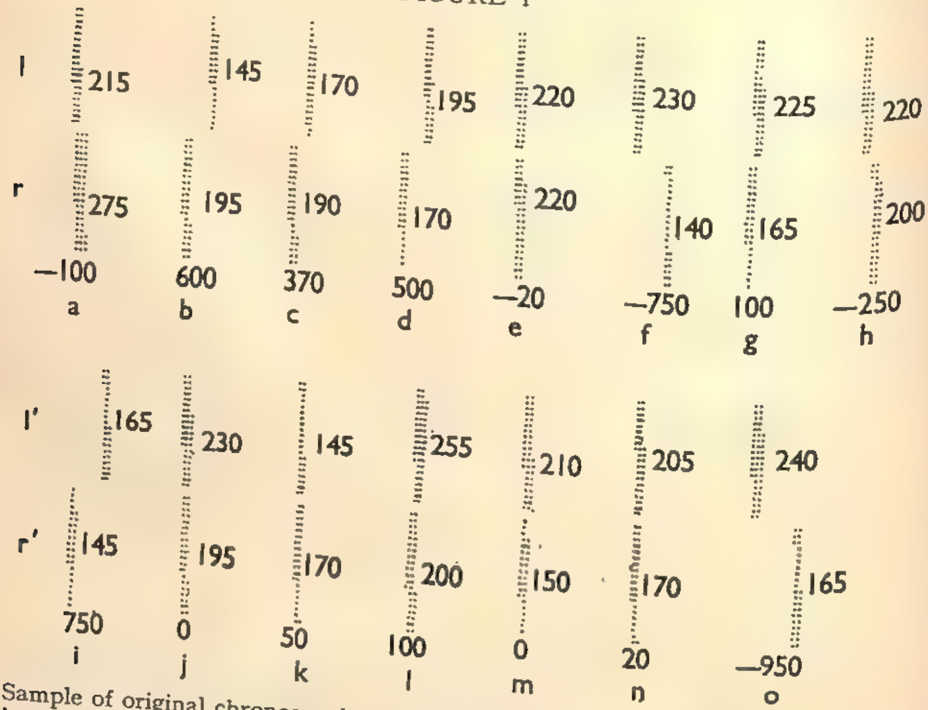
The reaction times were recorded by using Siemens high speed relays and a two-channel chronograph which was designed for this type of reaction time study by Dr. G. D. Dawson. This instrument has been described elsewhere (Dawson and Elithorn 1953) and a sample of the records obtained is shown in Figure 1.

Results

Figure 1, which is a reproduction of the record of fifteen consecutive paired responses made by subject J.P., shows an example of the original data and illustrates the variability of the responses. The principle and construction of this

chronograph have been described elsewhere. In essence it prints—between the onset of the stimulus and the opening of the reaction key—a series of dots. These dots are arranged in columns of twenty and are printed sequentially at intervals of 5 m.s.

FIGURE 1

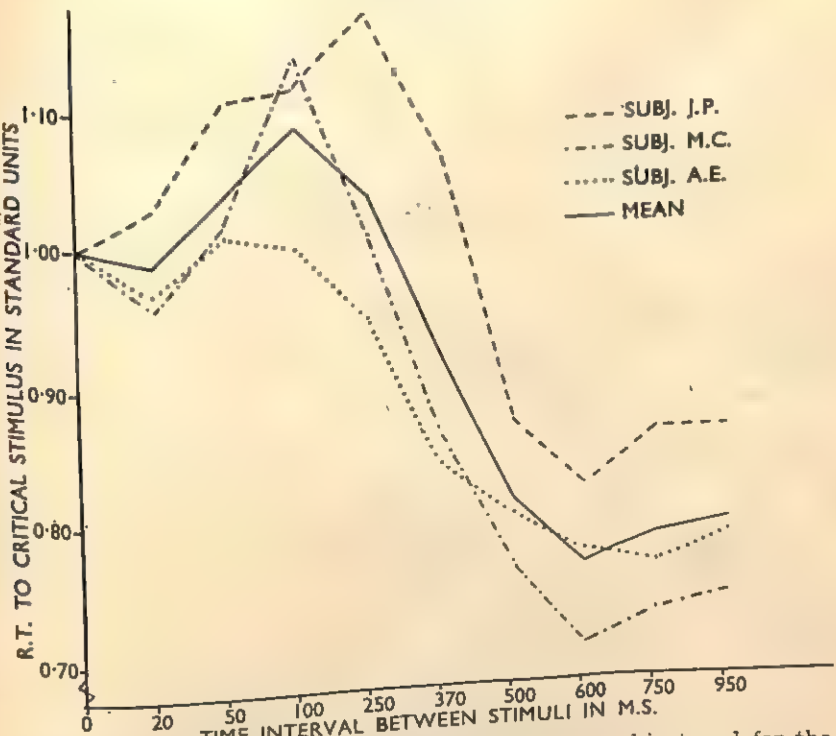


Sample of original chronograph record showing R.T.s to left-sided stimuli (rows l and l′) and right-sided stimuli (rows r and r′). The second part of this record has been reproduced below the first part with which it was originally continuous. The time interval between the two stimuli has been written in beneath each pair and is negative when the right-hand stimulus was second.

The individual reaction times to the nearest 5 m.s. can rapidly be found by inspection and in the present instance they were written in on the record on the right side of each set of dots. In this record the top row of dots *l* and *l′* represents the R.T.s for the left sided stimulus response situations and the lower row of dots *r* and *r′* the R.T.s for the right sided situations. Beneath each pair of responses has been written in the time interval in milliseconds between the two stimuli. As a convention these have been written with a negative sign when the right-hand stimulus was second. In this sample record the right-hand stimulus was first on eight occasions, the left-hand stimulus was first on five occasions, while on the remaining two occasions the left-hand stimulus though second according to the randomization (i.e. a S_c situation) was presented simultaneously with the right stimulus, i.e. with a delay of 0 m.s. This subject was strongly right handed and in comparable situations the right sided responses were quite definitely faster than the left. In the right hand the responses, rows *r* and *r′* are, when the right stimulus is first, fairly fast:—situations *b*, *c*, *d*, *g*, *i*, *k*, *l*, and *n*, R.T.s 195, 190, 170, 165, 145, 170, 200, and 170 m.s., but appear to be distinctly faster when this hand is second by a good margin:—situations *f*, and *o*, R.T.s 140 and 165 m.s. When the intervals are short, however, the R.T. for the right hand when the right stimulus is second are relatively long:—situations

a, *e*, and *h*, R.T.s 275, 220, and 200 m.s. Similarly for the left hand the reaction times for the second stimulus are relatively fast when this stimulus is second by 500 m.s. or more:—situations *b*, *d*, and *i*, R.T.s 145, 195 and 165. At the intermediate time intervals there is clear evidence of delay, situations *g*- and *l*, R.T.s 225 and 255 m.s., but this is not obvious at the shortest time intervals, situations *k* and *n*, R.T.s 145 and 205 m.s. These latter times are in fact faster than those recorded when the left stimulus was first:—situations *a*, *e*, *f*, *h* and *o*, R.T.s 215, 220, 230, 220, 240. These observations culled from a selected sample of the original record are not intended to be other than illustrative and they will be discussed later in association with the presentation of the results as a whole.

FIGURE 2

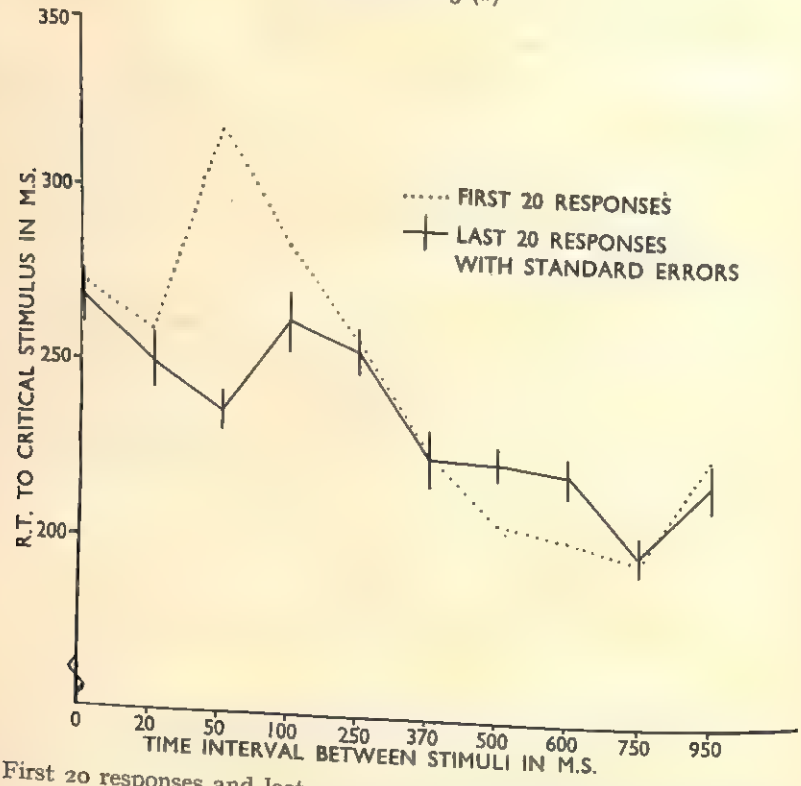


Graph of mean R.T.s to the S_c situation for each subject and for the group as a whole adjusted to a common origin at time o (*see* text).

Because the number of experimental sessions which could be obtained was different for each subject and because the occurrence of each situation in the series was random the number of observations made at each time interval varied considerably. The data as a whole is therefore presented graphically, the relevant statistical checks being described in a later section. The mean reaction times for the S_c situations for each of the three subjects and for the group as a whole are presented in Figure 2. In order to reduce the variation between individuals the mean R.T.s are presented as a function of the mean response time for S_c when this stimulus was presented simultaneously with the S_1 stimulus. That is to say, unit time for the S_c response has been regarded for each subject as the mean observed S_c reaction time at time o—i.e. when S_c accompanied S_1 without delay.

For clarity and since the mean concordance (Kendal 1948) between the hands of the individual, is greater, $W = 0{\cdot}911$, than the concordance between individuals, $W = 0{\cdot}903$, the results for each hand have been combined to give an overall mean for each subject at each time interval. Although n is not constant the number of observations from which each mean is derived is never less than 56 for subject J.P., 45 for subject M.C., and 103 for subject A.E. The mean results for the group as a whole is represented by the uninterrupted line; for this group the number of observations at any time interval was never less than 222. It is apparent from the summed result that there is in this situation as in Vince's some delay in initiating the response to S_c when this stimulus follows shortly after S_1. Contrary however to the expectations engendered by the theory of the "psychological refractory state" this delay is maximal not at the shortest intervals but at the 100 m.s. interval.

FIGURE 3 (*a*)



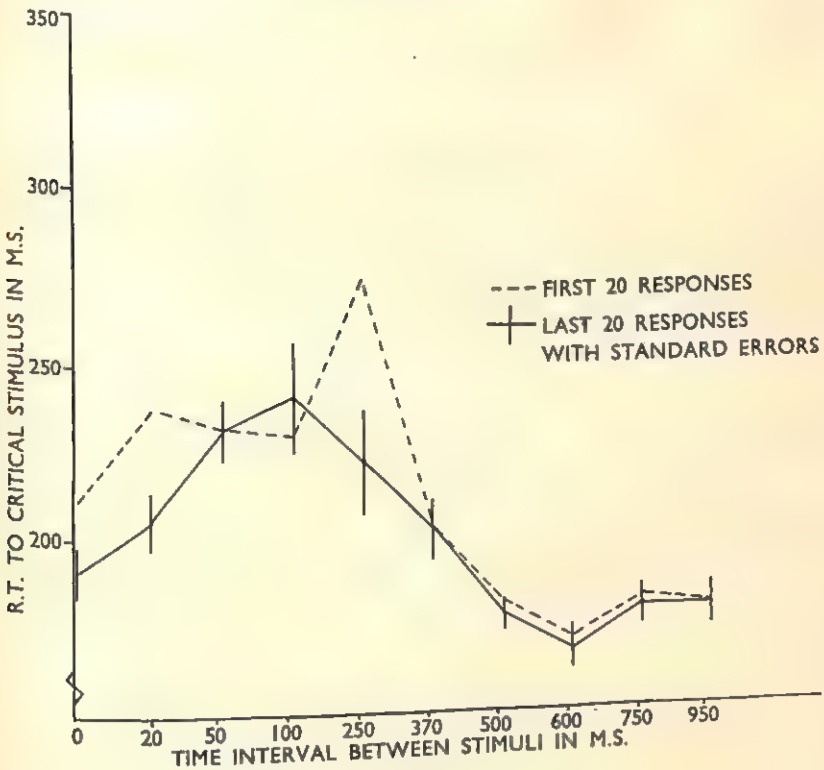
First 20 responses and last 20 responses with their standard errors of subject A.E. (right-hand) showing disappearing inhibition with practice.

Examination of the individual results shows that in two of the three subjects this inhibition was marked but that the effect was small in the third. This latter subject had had most practice in the present situation and also considerable previous experience of step-tracking situations similar to those investigated by Vince. It seemed possible that his lack of inhibition might—again contrary to Welford's expectations—be due to practice. Examination of the detailed data showed that this was at least partly the case. Figure 3*a* shows for subject A.E.'s right hand the mean times for the first 20 and the mean times and standard errors for the last 20 R.T.s. There is

clear cut inhibition during the first 20 R.T.s which has disappeared by the last 20 R.T.s. The left hand, however, (not illustrated) shows little inhibition even at first but a similar practice effect can still be seen. Practice effects in the other two subjects have also been sought.

Subject J.P. for whom similar data is given showed similar practice effects. In this case inhibition was still present during the last 20 reaction times, but it had been reduced in extent and revealed its peak at an earlier time than at the beginning of the experiment. The third subject M.C. did not, with either hand, complete 40 responses at each time interval but the mean responses for the first and last ten situations at each site for the right hand are given in Figure 3*c*. These results are of interest in that they show an all-round improvement in performance without much alteration in the shape of the curve.

FIGURE 3 (*b*)



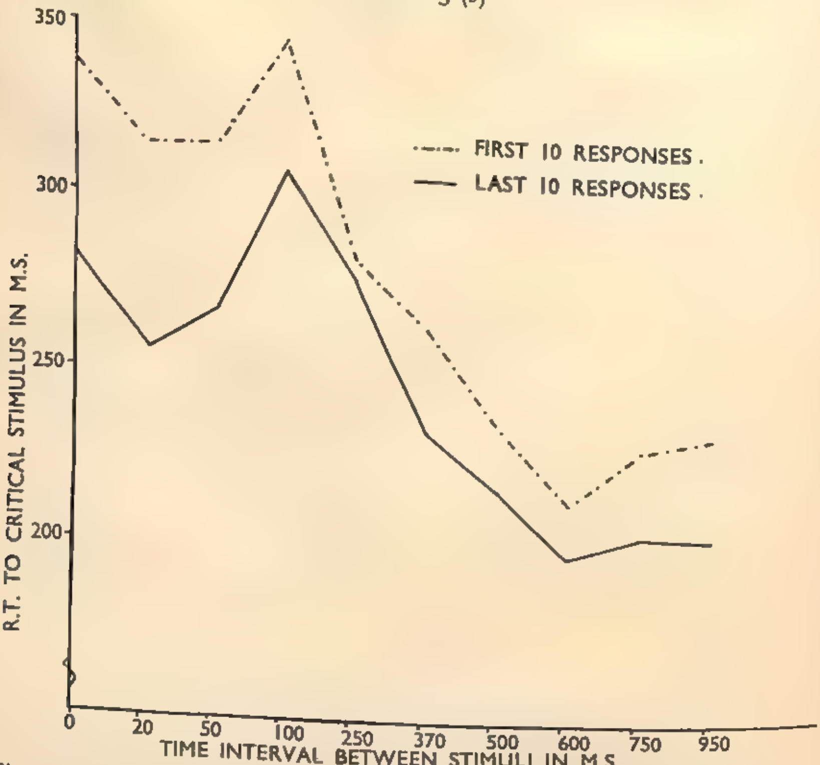
The same for subject J.P.

Control Observations.

These are restricted in extent since the occasions when situation S_1 was omitted were limited to one in five. Nevertheless these observations do provide for each subject an estimation of the time taken to respond to S_c when it was known that S_1 would not occur. This control was therefore a single disjunctive situation in which the time interval between the warning signal and the stimulus was varied in the same manner as in the main experiment. The differences between the R.T.s for the second situation observed in the main experiment and those observed in the control period reflect therefore the effect on a disjunctive reaction time of performing one of the

alternative responses at time 0. These differences are given in Table I. With the exception of the left hand responses of subject A.E., the most practised subject, all six disjunctive stimulus response situations S_c were performed less speedily when the alternative response S_l was performed at time 0 and the signal S_c was presented within the first 250 m.s. This interference, however, was not maximal immediately after the presentation of the distracting stimulus but was greatest when the second stimulus was presented after a delay which varied between 50 and 250 m.s. With delays which were longer than 500 m.s. the reaction times in the main experiment were, for each hand of each subject, shorter than those for the control observations. Presumably this was because the presentation of S_l had converted the disjunctive situation into a simple one. This interpretation is further supported by the observation—in one subject—that the simple reaction times observed when the subject knew on which side to expect the stimulus were markedly shorter than his disjunctive reaction times.

FIGURE 3 (*c*)



First and last 10 responses (right-hand) of subject M.C. showing general improvement in length of R.T. without change in the shape of the curve.

Errors.

Although a detailed study of premature reactions and false responses has not been made their frequency was low and not sufficiently great to invalidate the results obtained. For the three subjects J.P., M.C., and A.E. the respective error rates were 3·0 per cent., 1·5 per cent. and 1·4 per cent. Inspection of the individual data showed that the majority of false responses tended to occur late in the situation when the subject was awaiting the second stimulus which by then possessed a rapidly increasing

probability. It is a generally accepted thesis that false reactions tend to occur when expectancy is high and it therefore lends definite support to our interpretation of central inhibition is a function of expectancy (see discussion) to observe that "refractoriness" (Fig. 2) was most marked in the subject with most errors J.P. and least marked in the subject with least errors, A.E.

TABLE I

Subject	*Hand*	*Time Interval*									
		0	20	50	100	250	370	500	600	750	950
J.P.	L.	+7·0	0·	+6·3	+0·9	+29·6	+45·9	−24·9	−44·8	−28·0	−27·0
	R.	+5·9	+0·7	+21·5	+9·0	+69·6	−6·9	−26·4	−38·6	−17·7	−31·4
M.C.	L.	+40·0	+25·9	+35·6	+77·3	+40·3	+28·7	−5·8	−42·6	−38·0	−54·8
	R.	+35·6	+40·7	+30·4	+114·9	+46·8	+17·7	−14·3	−38·6	−32·1	−48·5
A.E.	L.	+0·3	−13·4	−9·6	−16·8	−4·9	−40·4	−43·5	−61·5	−55·1	−45·5
	R.	+26·1	−6·9	+28·1	+5·2	−18·7	−4·7	−32·1	−31·5	−36·4	−23·2
Mean Difference		+19·2	+7·8	+18·7	+31·8	+27·1	+6·7	−24·5	−42·9	−34·6	−38·4

TABLE II

Subjects	*Time Interval between Stimuli*									
	0	20	50	100	250	370	500	600	750	950
	R.T. To S_c (Right Hand) Mean of last twelve responses									
J.P.	191·7	199·6	231·7	238·8	197·1	194·1	177·1	168·8	170·8	164·6
M.C.	270·4	256·3	259·2	310·4	277·5	236·7	215·0	195·4	202·1	205·5
A.E.	264·2	245·1	241·3	257·1	245·8	236·3	225·5	222·1	204·2	217·1

Statistical Analysis.

Although these results are not, as is demonstrated in the discussion, in conflict with those of Vince and Hick they are incompatible with the theoretical formulation we set out to test. It would appear obligatory therefore to provide statistical support for the significance of our findings. Accordingly an analysis of variance has been carried out on the data in Table II which gives for the right hand of each subject the mean of the last 12 observations in m.s. when this hand was responding to stimulus S_c. In order to make the analysis more sensitive and to be able to test the interaction between the two main factors, Subjects and Time Intervals, a replication factor has been introduced by distributing each set of 12 observations randomly between two equivalent sets of six. A random distribution has been used to avoid contamination of the replication factor by practice effects. The results of this analysis are given in Table III. This analysis shows with a high level of confidence ($p<0{\cdot}001$) that the reaction times for S_c are a function of the time interval and that this function is different for different subjects. The characteristics of this function which vary with practice and between individuals cannot be established on the present data but statistical support is provided for two of the individual curves in Figures 3*a* and *b* where the standard errors of the means are also plotted. Consideration of these curves together with the data given in Tables I and II shows that in the present experiment the inhibitory effect of S_1 on S_c is not maximal when the time interval

separating the two situations is minimal but at some later time. Such an observation which is critical for the theory under consideration should of course be confirmed.

TABLE III

Source of Variance	*D.F.*	*Sums of Squares*	*Mean Square*	*Probability*
Between S (Subjects) ..	2	29225·67	14612·84	0·001
Between T (Time Interval)	9	36041·98	4004·66	0·001
Between AB (Replication)	1	26·66	26·66	N.S.
AB × S Interaction ..	2	342·23	171·12	N.S.
AB × T Interaction ..	9	1640·88	182·32	N.S.
S × T Interaction	18	9981·70	548·98	0·001
Residual	18	1623·06	90·17	—
Total	59	78882·18		

Discussion

Welford's theory implies that the delay in the reaction time to the second of a pair of stimuli will be longest when the pair of stimuli are closest together. With Vince's data this is the case but it is not so with the results of the present experiment. In one subject the reaction time to the second stimulus is longest when the separation between the stimuli is 50 m.s. In the other two subjects this interval is 100 m.s. and 250 m.s. respectively. For the group as a whole the delay is maximal at 100 m.s. (see Fig. 2).

Although these observations are limited in extent they are in this respect consistent and would appear to be incompatible with the hypothesis which the experiment was designed to test. They do however conform with the observations of previous workers in that the reaction time to the second stimulus of a pair is *often* longer than that to the first.

In view of the small extent of our observations it is not our intention to develop any original postulates but it would seem worth while however to consider the interpretation of this inhibitory phenomenon in terms of expectancy theory as developed by Mowrer (1938). In his introduction Welford dismisses any explanation of the psychological refractory period in terms of "expectancy" on the grounds that this merely shifts the problem to the question: "In what does expectancy consist." We have found, particularly in dealing with pathological psychological states that Mowrer's concept of "expectancy" is both useful and as far as it goes, theoretically sound. (Elithorn *et al.* 1955.) It is our opinion therefore that if the refractory period can be interpreted in terms of a theory of expectancy then such an interpretation may aid not only our understanding of the refractory period but may also contribute to our understanding of the mechanisms underlying the formation of anticipatory sets.

In a reaction time situation the probability of a given motor response occurring at any particular time is closely related to the probability of the presentation, a reaction time earlier, of the appropriate stimulus. That the reaction time will increase as this probability decreases was originally demonstrated by Merkel and has recently been confirmed by Hick (1952). Similarly Mowrer (1940) has shown that when the stimulus for a simple R.T. occurs at a varying time interval after the warning signal the R.T. observed will be shortest at the time interval with the highest conditional probability. Again a simple R.T. will increase as the time interval during which the stimulus may be expected becomes longer (Woodworth 1938). In the

present analysis therefore it is presumed with these authors that the reaction time will decrease as the state of preparedness, excitability or probability of the appropriate motor response increases.

For the purpose of the present discussion a stimulus with which a motor response is associated will be regarded in relation to this response as an excitatory one. Similarly it would seem reasonable that a stimulus which elicits an alternative response and which indicates that the response under consideration is not likely to be immediately required, would have an inhibitory effect on the excitability of the motor mechanisms responsible for elaborating this response.

In the two stimulus response situations described in the present experiment the two tasks have a high degree of independence and as far as external space is concerned there is no overlap at all. Internally this independence is maintained for the peripheral nervous processes both sensory and motor. Even within the neuraxis this distinction remains to some extent as there are good grounds for thinking that the ablation of one hemisphere would abolish one response while leaving the other relatively unimpaired. At a higher level still it is worth noting that a lesion in a visual association area can cause a disturbance in visual attention which is related predominately to the opposite half field. (Brain 1951.)

If now we consider the state of preparedness or excitability of the two motor responses involved in the present study it is not unreasonable to suppose that during the fore period and during the period when the warning light is presented this excitability increases equally on the two sides. With the onset of the first signal the state of excitability of the appropriate response mechanism will increase rapidly until it reaches a threshold value and the response occurs. This stimulus, however, also indicates that the alternative response is not required at time o as half-expected but will be required at some later time during the next second. Further if this first signal has active inhibitory value for the response that is unwanted at time o then it is clear that both response mechanisms can be raised, by the warning signal, to a higher level of excitability without increasing the danger of error than would otherwise be possible. This level of preparedness or excitability will correspond to that of a disjunctive reaction time and presumably neither response will be able to attain that degree of preparation which can be achieved in a simple reaction time situation.

This argument is perhaps clearer in terms of probabilities. Thus at time o at the end of the warning period one of two responses will almost certainly be required and the probability of each stimulus-response situation occurring then is approximately 0·5. The arrival of a single stimulus raises the probability of its own response to the region of 1·0 but at the same time it causes the probability of the alternative situation to fall abruptly. Since this latter situation will arise sometime the probability that it will occur at the next time interval if it has not already occurred will increase with the passage of time until towards the end of the cycle this probability approaches unity.

If, therefore, the observed reaction time to the second stimulus is inversely proportional to the hypothetical state of preparation, expectancy or probability of the appropriate response mechanisms, then it may be expected to be longer when this stimulus occurs shortly after the disjunctive situation. Similarly it may be expected to approach that of a simple reaction time as the interval between the stimuli increases. Because the process of inhibition or whatever physiological change accompanies a sudden alteration in probability level may be presumed to take a finite time this central inhibitory state will not reach its maximum until some time after the arrival of the first stimulus. This hypothetical relationship between the probability of the stimulus-response situation and the state of preparation is illustrated somewhat crudely in Figure 4.

In the case of the two stimuli situation reported by Vince and discussed in detail by Welford the first reaction does not appear to be a choice reaction and the second stimulus-response situation has a limiting characteristic in that the second stimulus cannot be generated until the first stimulus has occurred. Under these conditions it will be apparent that during the time when the first stimulus is expected the state of preparedness of the relevant response will be at its greatest value and that of the second response relatively low. An additional complication is present in that the movements required for the two responses are antagonistic. That is to say that execution of the first response requires the relaxation or inhibition of tone in muscles which are the prime movers of the second response. Since a state of preparatory excitation would seem to be incompatible with a state of active inhibition, it follows that the state of excitation of the second response mechanisms at time o (time of first stimulus) is likely to be much lower than if the two responses were not incompatible.

FIGURE 4

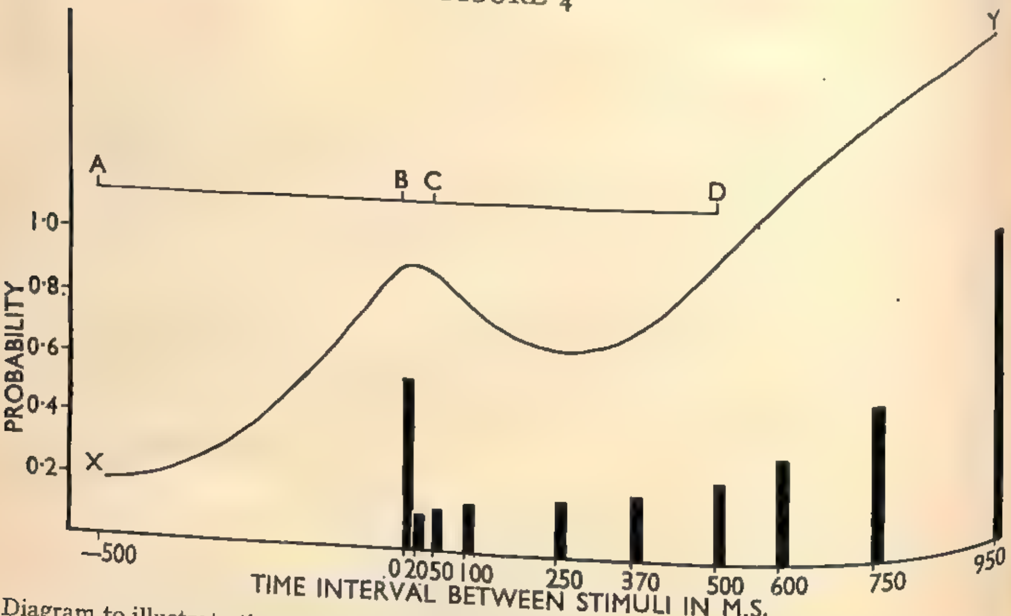


Diagram to illustrate the hypothetical state of preparedness of a response and its relationship to the probability of the appropriate stimulus arriving at the organizing centres. The histogram along the bottom of the diagram shows for each of the ten time intervals in one cycle the probability that a stimulus will occur at the indicated interval given that it has not already occurred. The curve X.Y. represents in arbitrary units the state of preparedness of the motor response and therefore bears an inverse relationship to the reaction time. During the warning period AB the state of preparedness is increasing rapidly and equally for the two responses and at B (time o) it is that of a choice reaction time. With the failure of the expected stimulus to arrive, inhibition starts, but takes an appreciable time to halt the excitatory process so that there is a short period BC during which the state of preparedness is greater than that found at time o. During the period CD inhibition has reduced the excitatory state to a level below that of the choice reaction time. From D onwards, however, the state of preparedness rapidly approaches that of a simple reaction time with a fixed fore-period.

Thus the state of preparation of the first response will be that of a simple reaction time with a variable foreperiod while that of the second response will be minimal until the first stimulus has arrived. Because of the special circumstance that the same motor

mechanisms are involved with contrary sign by both responses the second response might by a process of reciprocal inhibition be unable to accept any excitatory impulses until after the discharge of the first response. Such a refractory state of the motor mechanisms would it is true add to the observed delay in the second reaction time but it is specifically and rightly rejected by Welford as an example of "central inhibition." It may nevertheless be a valid and common cause of intermittent behaviour by the human operator.

Interpreting Vince's situation in terms of probabilities it is clear that the likelihood of the first stimulus response situation arising is similar to that in a simple reaction time situation in which the foreperiod is variable. The probability of the second stimulus response situation however is contingent on that of the first stimulus, since until the first stimulus has been generated there is no chance of the second arising. With the arrival of the first stimulus the probability or expectation of the second situation increases rapidly and approaches that found in a simple reaction time situation when the foreperiod is variable.

It is clear therefore, again with the proviso that physiological processes have extension in time, that in the situation examined by Vince the reaction time to the second of a pair of stimuli will be longer than that to the first member of the pair. Because the first signal carries no inhibitory sign for the second response—indeed it acts as an excitatory warning signal—this delay will be maximal when the second stimulus arises immediately after the first and will decline and eventually disappear as the time interval between the stimuli increases.

The above discussion is intended to be stimulating rather than exhaustive. It is hoped that it shows that a two stimulus reaction time situation often cannot be treated as an interaction between two identical situations since the occurrence of one stimulus will affect the anticipatory set with which the second stimulus must interact.

We are particularly indebted to Dr. G. D. Dawson who designed and supervised the building of most of the apparatus, and to Dr. E. A. Carmichael and Professor O. L. Zangwill for criticism and encouragement. To Dr. P. Armitage we are grateful for statistical advice. To Dr. J. A. V. Bates and Mr. M. F. Piercy we are indebted for criticism of the original manuscript.

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A TECHNIQUE FOR COMPARING THE THRESHOLD CONCENTRATIONS FOR OLFACTORY, TRIGEMINAL, AND OCULAR IRRITATIONS

BY

R. W. MONCRIEFF

From the Consulting Laboratory, 83 St. Pancras, Chichester, Sussex

A new technique is described for comparing the threshold concentrations that will produce olfactory, trigeminal, and ocular irritation when odorous irritants are exposed to the nose and eyes. It depends on the reduction of concentration of an odorous irritant in air, when the air is passed through an adsorbent.

Of the four odorous irritants used in this investigation, three, viz. acetic anhydride, formaldehyde and triethylamine, first irritated the eye, then became perceptible to smell and finally irritated the nasal membranes as their concentration was gradually increased. The order of sensitivity of the three senses towards them was in each case: eye irritation > smell > nasal irritation. It was not found possible to separate the three sensations when ammonia was the odorous irritant, all three seeming to appear and to disappear together.

Introduction

Those odorous substances which are also irritant will excite not only the olfactory receptors but also the free endings of the trigeminal nerve. According to the location of the irritation, whether it be eyes, nose, throat, or anus, we experience different sensations and it is evident (Moncrieff, 1951) that there are different modalities of the common chemical sense just as there are, for example, of the sense of taste. Pungency, lachrymatory properties and sternutatory properties are examples of characteristics which stimulate different modalities of the common chemical sense.

When one substance possesses, as many do, not only a true smell but also an irritant property, it is not always easy to decide the exact incidence of the two separate stimulations; rather does it often seem that both smell and irritation come and go together. The most important published work on the subject is that of Katz and Talbert (1930) who measured the threshold concentrations of 55 different substances for odour, nasal irritation and eye irritation; they found, amongst other things, that such odorants as artificial musk could be smelt in concentrations that were very much lower than those at which even powerful irritants such as chloracetophenone exerted any physiological action; smell could be much more sensitive than trigeminal or ocular irritability.

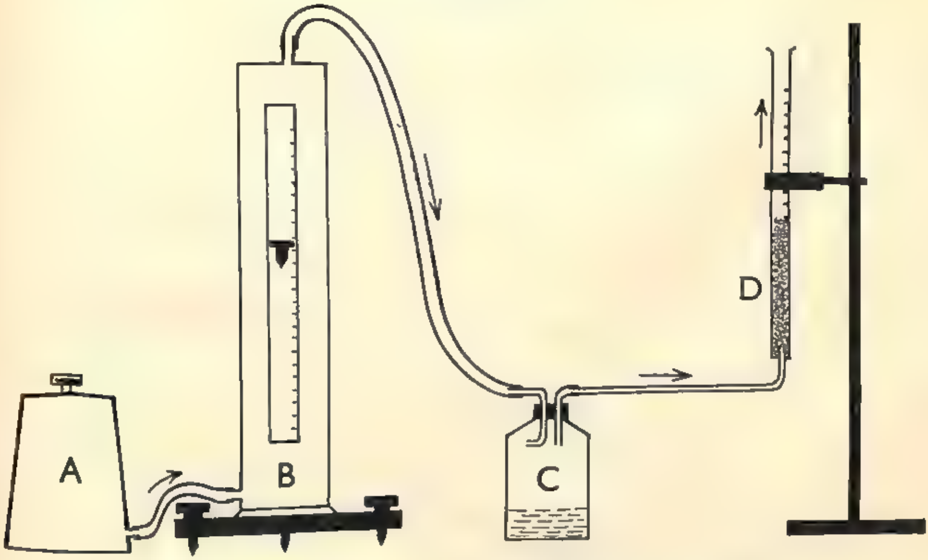
This paper is concerned with a new technique for comparing the threshold concentrations for olfactory, trigeminal, and ocular irritations. It is applied to four substances all of which cause these several irritations, and it depends on the progressive increase in the rate at which air containing an odorous irritant substance is passed through an adsorbent; the incidence of each of the three irritations, olfactory, trigeminal, and ocular, is observed and a comparison is made of the minimum times of contact of the air containing an odorous irritant with an adsorbent necessary just to prevent each of the three sensations.

Methods and Apparatus

The apparatus, Figure 1, used was similar in principle to that which had been devised for a new approach to the characterization of odours (Moncrieff 1954). In the present investigation, air containing an odorous irritant was blown at a known rate through an

adsorbent, and the times of contact with the adsorbent which were necessary to remove respectively (*a*) the odour, (*b*) the nasal irritant action, and (*c*) the eye irritant action from the odorized air were measured. In this way it was possible to compare the sensitivities of the three sensations—the longer the time of contact of the odorous air with the adsorbent the lower the concentration of odorant in the air issuing from the adsorbent.

FIGURE 1



A = Air Blower.
B = Flow-meter.
C = Bottle of Odorous Irritant.
D = Adsorption tube.

Diagrammatic representation of apparatus for determining critical time of contact for adsorbent and odorant.

In practice an electrically driven air-pump, A, (Fig. 1) blew air through a Rotameter flow-meter, B, at speeds of from 10 c.c. to 50 c.c. per second. This air was passed over an odorous irritant in bottle C, and then up a graduated glass tube, D, of 0·5 cm^2. diameter which was packed to a known height (a suitable height was determined by one or two pilot runs) with the adsorbent material which was either active carbon or silica gel. The top of the tube, D, was inserted into one nostril and the air blower started and its speed gradually increased until the smell of the odorous irritant was just recognizable with certainty at the outlet end of the tube, D. A similar experiment was next made in which the air speed for the first tingling sensation in the nose was observed and a third in which the top of the tube, D, was cupped by the hand and the eye placed to the cup. In the case of eye observations, the eye was winked steadily as it was found that when this was done the eye was more sensitive and that more consistent results were obtained; if it was not done, then an involuntary wink would considerably modify the critical air speed. Each observation took about 20–30 sec. and as a fresh charge of adsorbent was used for each observation, there was no chance of its performance being seriously affected by the amount of odorant it had already adsorbed.

Results and Discussion

Acetic anhydride.

Acetic anhydride was the odorous irritant first used. It has a distinct acetic odour, an aggressive action on the eyes and it causes tingling of the mucous membrane in the nose.

The apparatus shown in Figure 1 was used, 50 ml. acetic anhydride being placed in the 6 oz. bottle C. The adsorbent used was active carbon (Sutcliffe Speakman and

Co. Ltd., Leigh, Lancs., 208 C 6–10 mesh). Experimental results were as shown in Table I.

TABLE I

ADSORPTION OF ACETIC ANHYDRIDE BY ACTIVE CARBON

Height of column of Active Carbon (cm.)	*Air speed (c.c. per sec.) at which there could just be perceived:*			*Corresponding critical time (sec.) of contact of odorized air with active carbon to allow perception of:*		
	Smell	*Nasal Irritation*	*Eye Irritation*	*Smell*	*Nasal Irritation*	*Eye Irritation*
1·8	12·5	16		0·072	0·056	
2·1	16	19		0·066	0·055	
2·4	16	22		0·075	0·055	
3·2	27	26		0·059	0·062	
3·7	27	40		0·069	0·046	
3·3			17·5			0·094
3·8			16·5			0·115
3·9			17			0·115
4·0			17·5			0·114
4·8			22			0·109
Mean critical times of contact (sec.)				0·068	0·055	0·109

Eye irritation is perceived first, then smell, and lastly as the air speed is increased further so that the time of contact with the adsorbent is correspondingly reduced, nasal irritation.

TABLE II

ADSORPTION OF FORMALDEHYDE BY ACTIVE CARBON

Height of column of Active Carbon (cm.)	*Air speed (c.c. per sec.) at which there could just be perceived:*			*Corresponding critical time (sec.) of contact of odorized air with active carbon to allow perception of:*		
	Smell	*Nasal Irritation*	*Eye Irritation*	*Smell*	*Nasal Irritation*	*Eye Irritation*
2·1	14·5	18	10·5	0·072	0·058	0·100
3·0	17·5	22	17	0·086	0·068	0·088
4·0	24	35	17·5	0·083	0·057	0·113
4·1	27	33	18	0·076	0·062	0·114
4·6	27	38	26	0·085	0·061	0·088
Mean critical times of contact (sec.)				0·080	0·061	0·101

Formaldehyde.

Formaldehyde has an irritant action on the nasal membranes and on the eyes as well as having a characteristic odour. It was used in this experiment which, apart from the substitution of formaldehyde (40 per cent. solution in water) for acetic anhydride, was run as described above with the apparatus shown in Figure 1. Experimental results were as shown in Table II.

Eye irritation comes first, then smell and lastly nasal irritation.

Ammonia.

Ammonia was used as the odorous irritant. Preliminary trials were made using active carbon as the adsorbent, but the adsorption was so poor that silica gel (6–8 mesh Silica Gel Ltd., London) was tried; it was found to be more satisfactory and was used (instead of active carbon in tube D) for the adsorption of ammonia.

The apparatus shown in Figure 1 was used, a 15 per cent. solution of ammonia in water having been introduced into bottle C, and measurements were made as described for the earlier experiments. Experimental results were as shown in Table III.

TABLE III

ADSORPTION OF AMMONIA BY SILICA GEL

Height of column of Silica Gel (cm.)	*Air speed (c.c. per sec.) at which there could just be perceived:*			*Corresponding critical time (sec.) of contact of odorized air with silica gel to allow perception of:*		
	Smell	*Nasal Irritation*	*Eye Irritation*	*Smell*	*Nasal Irritation*	*Eye Irritation*
3·9	11	11	11	0·177	0·177	0·177
5·6	18	18	18	0·156	0·156	0·156
6·8	16	16	16	0·213	0·213	0·213
7·8	19	19	19	0·205	0·205	0·205
9·0	27	27	27	0·167	0·167	0·167
Mean critical times of contact (sec.)				0·184	0·184	0·184

In this experiment it was found to be impossible to separate the appearance of the true odour from that of nasal and eye irritation. All three perceptions appeared together.

TABLE IV

ADSORPTION OF TRIETHYLAMINE BY ACTIVE CARBON

Height of column of Active Carbon (cm.)	*Air speed (c.c. per sec.) at which there could just be perceived:*			*Corresponding critical time (sec.) of contact of odorized air with active carbon to allow perception of:*		
	Smell	*Nasal Irritation*	*Eye Irritation*	*Smell*	*Nasal Irritation*	*Eye Irritation*
8·0	16	24	17·5	0·250	0·167	0·229
8·8	16·5	22	17·5	0·267	0·200	0·251
9·0	16	25	17	0·281	0·180	0·264
9·9	16·5	27	18	0·300	0·183	0·275
12·5	19	35	20	0·329	0·179	0·313
Mean critical times of contact (sec.)				0·285	0·182	0·266

Triethylamine.

In view of the unexpected and apparently odd behaviour of ammonia, it was decided to use a not altogether dissimilar type of substance but one with a more

organic nature for this experiment. Triethylamine was chosen; 50 ml. of it was introduced into bottle C (Fig. 1) and air was blown over it. Active carbon was used as the adsorbent in Column D, as it was found to retain the triethylamine satisfactorily. Silica gel was also tried and was similarly satisfactory. As, however, it was possible to use the carbon, it was preferred to do so, to facilitate comparison with earlier results. Experimental results were as shown in Table IV.

Towards triethylamine, smell is the most sensitive of the three senses, with eye irritant perception nearly as sensitive and with nasal irritant perception considerably blunter.

The critical times of contact with the adsorbents of each of the four odorous irritants necessary to prevent olfactory, trigeminal, and ocular irritation respectively are assembled in Table V.

TABLE V

CRITICAL TIMES OF CONTACT OF ODOROUS IRRITANTS WITH ADSORBENTS

Air odorized with	*Adsorbent*	*Critical time of contact (sec.) of odorized air with adsorbent for:*		
		Smell	*Nasal Irritation*	*Eye Irritation*
Acetic anhydride	Active carbon	0·068	0·055	0·109
Formaldehyde	Active carbon	0·080	0·061	0·101
Ammonia	Silica gel	0·184	0·184	0·184
Triethylamine	Active carbon	0·285	0·182	0·266

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THE PERCEPTION OF THREE-DIMENSIONAL SOLIDS

BY

J. LANGDON

From the Institute of Experimental Psychology, University of Oxford

Arising from the discussion of differences between three- and two-dimensional perceptual objects in relation to current theories, an attempt is made to extend the traditional type of shape-constancy investigation into the realm of three-dimensional solids. For this purpose a specially constructed "solid" is made to undergo progressive physical changes of shape while being compared, under controlled conditions, with various stationary two-dimensional projections.

The results indicate that three-dimensional solids possess perceptual properties not shared by simple surfaces or representational projections. It is suggested that changes in the magnitude and sign of the constant errors obtained under certain conditions can be explained only on the assumption that subjects react to the stimulus in terms of some conceptual schema, or reference frame, involving mental processes other than those of perception.

I

Introduction

Almost all experimental studies of perception of shape and shape constancy have been carried out with the aid of two-dimensional surfaces or outlines, and their results applied to shape perception of objects in general. In all such studies, the method employed has been the orientation of some regular two-dimensional surface about an axis and its attempted comparison with an angular projection of itself located in the frontal plane. This technique has been used by Thouless (1931), Moore (1938), Stavrianos (1945) and Langdon (1951; 1953; 1955) among numerous others.

It has been pointed out by Gibson (1951) that many erroneous ideas as to the nature of perception may derive from the use of two-dimensional stimuli in such experiments when these results are attemptedly generalized to cover perception of solid objects. Nevertheless, in an earlier study Gibson himself (1950) appeared to assume an equivalence between perception of solids and perception of two-dimensional surfaces (pp. 170–171). Moreover, in a recent study, Gottheil and Bitterman (1951) have pointed out that results obtained by Thouless using the technique described above were to some extent outcomes of the method employed, in that the subject could never, in fact, match the shape of the oriented stimulus and its frontal-parallel projection. This criticism corresponds to that given of Thouless's results in the case of size constancy by Joynson (1949*a*: 1949*b*).

Bearing these observations in mind, it would seem worth while to examine the way in which shape perception operates, using actual three-dimensional stimuli in place of extrapolating results derived from simple surfaces employed as their surrogates; and secondly, to modify the actual shape of the stimulus object, in order to make it correspond with the projection, instead of merely changing its apparent shape by varying its orientation. Such an approach offers the possibility of inducing constant errors and perceptual compromises on the part of the subject, indicative of the main criteria governing his normal reaction, while at the same time it avoids a situation in which he vainly endeavours to equate euclidean and projective shapes.

II

Aim of Experiments

The primary aim was to extend the study of shape perception, carried out with two-dimensional forms, to cover three-dimensional solids. Since the situation this requires is new and different from those employing tilted discs and outlines, observations and the implications deriving from them go somewhat beyond the hypotheses the experiments were designed to examine.

On the immediate points at issue, the questions to be answered may be formulated as follows. Firstly, it has been suggested that it is illegitimate to generalize from results obtained with two-dimensional surfaces, applying these to perception of three-dimensional solids, since the two types of perceptual situation are not equivalent. On the contrary, it might be maintained that constancy of shape is dependent upon regular interlinkages between various planes of a solid preserved invariant through orientational transformations, so that the subject's reaction is qualitatively different in the two situations. The first problem is therefore to test experimentally whether the contention is correct.

Secondly, the reference position for calculating an index of phenomenal regression for shape constancy of two-dimensional objects has always been the diameter or major axis of the standard object. This cannot hold good for three-dimensional solids since in the case of the cube or parallelepiped when one face is frontal-parallel to the subject the other faces cannot be seen. Although Thouless attempted to define the "real" object to which the phenomenal regression occurred (presumedly demonstrated by the subject's constant errors of judgement), as "real" only in parenthesis, an "intuited" or conceptual object (Thouless, 1931, p. 358), nevertheless, using in practice a two-dimensional surface, the regression is shown as taking place simply to the projection of the surface lying in the frontal plane. The use of a three-dimensional stimulus, where this confusion cannot occur, may indicate more clearly what is involved in Thouless's notion of "phenomenal regression" to the "real" object, by showing whether this regression is from retinal stimulus to an "intuited" (i.e. a conceptual or abstracted) object, or merely to a most favoured pattern such as is obtained when the object is located in the frontal-parallel plane (*cf.* Koffka, 1935, p. 231 ff.). It will be necessary to return later to the theories advanced by Thouless, since his notion of an "intuited" object would appear to carry implications suggesting that these types of reaction go far beyond the recognized bounds of perception and involve other mental functions.

Third and lastly, there is the question of perspective, as distinct from orientation. Objects at different distances have different perspective and apparent shape is clearly a cue to size in addition to direct perception of distance. Hence, if perspective shape of a three-dimensional object is varied whilst its size and distance remain constant, will there be a change of apparent size or distance, a similar process of "phenomenal regression"? This is a separate question from the two first, since there is no suggestion of change in orientation or tilt, the perspective of the object being varied whilst it remains in the same plane.

These are the main questions the experiments below are designed to answer, using the technique of comparison figures matched with a three-dimensional standard capable of adjustment.

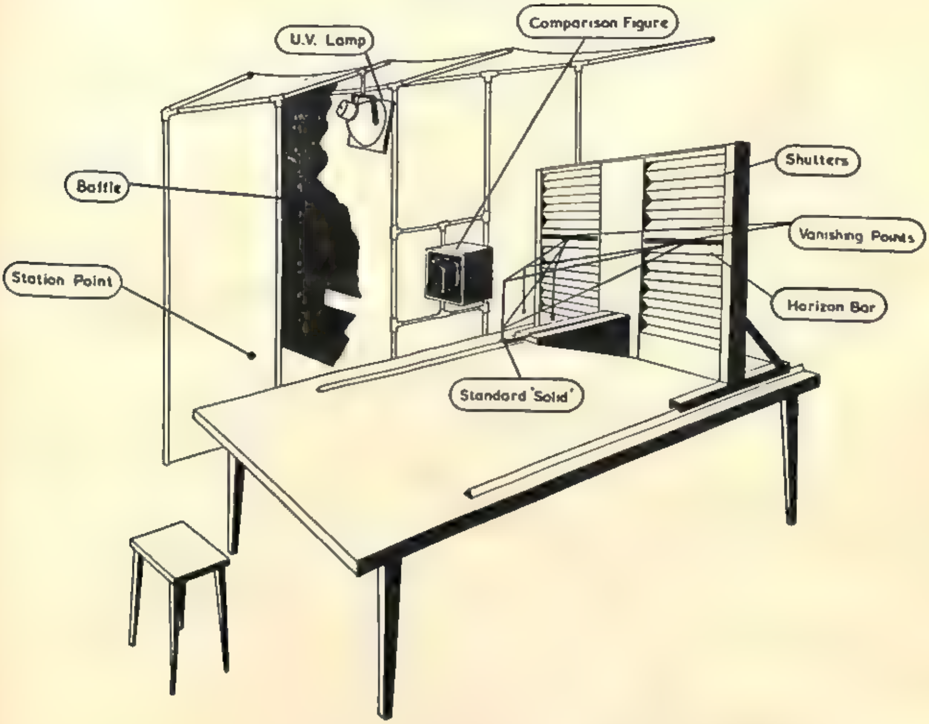
III

Method

All the experiments were carried out in a perfectly dark room measuring 20 by 15 feet. The stimuli were frameworks constructed of small-diameter tubing and made to fluoresce with U.V. radiation, and transparencies displayed in a box internally illuminated. In this way nothing but the stimuli could be seen, and their brightness could be controlled.

Figure 1 shows a general view of the apparatus. Its essentials consist of an array of stainless steel tubes 2 mm. in diameter, linked to form a parellelepiped presented in oblique perspective. The sides of this figure have a vertical/horizontal ratio of 4 : 5 and the included angles of the laterals may be varied to produce more or less perspective. The horizon may also be altered to correspond with Station Points at various elevations.

FIGURE 1



General view of apparatus (not to scale)

The geometrical construction is simple in the extreme. The lateral boundaries of the "solid" are six tubes passing freely through a left and right vanishing point. These are each flexibly mounted on a carriage which may be moved in simultaneous contrary motion along the f/p plane. All the junctions of the tubes forming the boundaries of the solid are pivoted so that its shape may be varied in accordance with different perspectives, horizontal and vertical. This structure is mounted on a carriage bearing a counterpoised horizon bar (containing the vanishing points) which can be raised or lowered at will and along this bar the vanishing points may be extended or brought together. The angle which they form at the leading edge of the solid may, if desired, be kept constant by advance or recession of the leading edge of the solid. The carriage is itself mounted upon a track along which it may be driven in either direction, thus maintaining a constant distance between the solid and the observer, despite advance or recession of the leading edge relative to the V.P's. The horizon and V.P's are raised, lowered, extended and retracted by means of electric motors which reverse automatically at the end of each horizontal or vertical excursion, as do those driving the carriage and the leading edge of the solid. The motors are controlled by thyratron-type circuits to give a wide range of speeds and can also be reversed manually at any desired point. All these operations are performed by relay circuits remotely controlled.

The "solid" has a fixed leading edge (i.e., its length does not alter and it always stays vertical) 6 inches high. At the junctions of the lateral sides and rear sides (lower rear corners and upper rear centre) the tubes are covered with short sleeves obscuring the portions of tube outside the figure. In this way, when the perspective is increased and

the rear verticals and cross horizontals become shorter, the surplus length of fluorescent-coated tube disappears into the surrounding sleeves. The rear vertical tubes are loaded and pivot freely from their junctions with the upper horizontals so that when the horizon bar rises and the main lateral horizontals tilt, the verticals remain upright and parallel with the stationary front post.

To obtain comparison figures, the three-dimensional solid was adjusted and set in twelve successive positions (See Fig. 2). A series of photographs was taken with a laboratory precision camera having universal front and rear movements. This enabled an accurate reproduction to be made of the solid by locating the camera at the point from which the subject would view the array during the experiment. The negatives thus obtained were enlarged on "Kodalith" sheets measuring 12 by 10 inches. This material has a very high density ratio and the black portion of the film is quite opaque. The perspective figure printed on this film is exactly the same size as the solid and is displayed in an internally illuminated box. The lamps inside the box were dimmed and the brightness and hue matched with the fluorescent solid. The box was mounted on a great arc 15° from the solid, the subject occupying a position 8 feet from both and with his eye level some 10 inches above the lower front corner of the solid. Thus with the horizon bar at a setting of 10 inches vertical elevation, the V.P's (horizon line) were on a level with the subject's eyes.

FIGURE 2

Twelve comparison figures (actual stimuli) with vanishing points at 21, 18 and 15 inch. Horizons at 7, 9 11 and 13 inch.

The subject was seated on a high stool without arms or supports so that he could gain no stray cues to his position in the room. In order to ensure that he looked from the desired point in space, baffles were so arranged that the stimuli could be seen only from one position, head movement or relaxing of his body obscuring part of the stimuli.

The scales used to measure the subject's estimates were purely arbitrary. They were simply the excursions of the vanishing points and horizon bar calibrated in inches. These can, of course, be converted into degrees of angle but there is little point in obtaining such data since only a relative comparison of one figure with another is required in carrying out the experiment, it not being the aim of this study to ascertain the absolute perspective values accepted as representative of an orthogonal solid. This would be a somewhat difficult task, as will be seen in due course.

IV

Description of Experiments I and II

The subject was led into the room (normally illuminated) and placed on the stool provided. His eye position was checked on a grid drawn on the wall and any adjustment of height required was made. He was unable to see the apparatus since it was entirely shrouded in black cloth stretched over a large frame 12 feet long and 10 feet high and having a curtain to the floor between the subject and the stimuli.

With the subject in position, a battery of bright lights was turned on for some minutes whilst the general purpose of the experiment was explained. This had the effect of accustoming the subject to high illumination, and delaying the onset of dark adaptation. All lights were then extinguished and the curtain drawn aside. At the outset, only the luminous "solid" was visible, the display box containing the comparison figure being switched off. The "solid" glowed as a faint green pattern in the dark space and the apparatus was then set in motion. First the horizontal perspective was changed at varying speeds, then the vertical perspective. This was repeated a number of times and the subject invited to comment on what he saw. After a few moments, the curtain was replaced, the lights turned on and the subject moved to a new position. From here, through additional holes in the baffles, the subject could now see (with lights extinguished and the curtain drawn back) the solid from a three-quarter oblique aspect. This meant that the leading edge now appeared slightly lateral and to one side, enabling the subject to describe apparent movement in what was previously his frontal-parallel plane. All remarks throughout the experiment were recorded on tape.

These preliminaries over, the main part of the experiment was begun. The subject was given a small hand-push button and asked to press it when the "solid" and the comparison figure (now visible) appeared most similar. The "match" was recorded by the experimenter from the scales at the rear of the apparatus. In the course of four separate sessions, each lasting about an hour, the whole family of comparison figures was displayed in random order and the solid matched with each one, both vertical and horizontal perspective being changed continuously at four different speeds. These ranged from 30 to 5 seconds for a vertical travel of 0 to 16 inches (horizontal to 30° inclination) and from 3 to 10 seconds for a horizontal travel of 8 inches.

Twenty-four persons, twelve men and twelve women, took part in this experiment. All used binocular vision, but a check experiment was carried out with six of the same subjects using monocular vision. The results are given in Tables I and II. Those obtained with monocular vision will be given in a later paper.

V

Results of Experiments I and II

From the tape recordings of subjects comments it was possible to assemble a general picture of how the "solid" was perceived, and it is worth discussing this before passing on to the quantitative results.

It will be appreciated that the plastic "solid" provides data which are, in a sense, contradictory to normal experience. Thus with rise and fall of the horizon line the upper face of the solid tilts just as it would were an ordinary solid being tipped forward. At the same time, the verticals remain in the same plane so that the perceived changes correspond roughly with what would be seen if the observer himself rose or fell, relative to the object, whilst this remained stationary. Similarly, with change of vanishing points the increased or decreased perspective ("foreshortening") corresponds to the change in apparent shape consequent upon advance or recession of object or subject. At the same time, the distance of the "solid" remains unchanged.

It would therefore be logical to expect subjects either to notice these disparities, to be unable to relate changes occurring in one part of the figure to those occurring in another, or to overcome the contradictions by perceiving what is in fact taking place, namely, plastic deformation of the "solid." In the event, subjects did none of these things. It must be emphasized that not one subject ever raised the possibility

of the shape's being plastic. Every observer accepted it as a rigid, solid body, falsifying the evidence of their senses in order to do so. This outcome is not merely due to ignorance of the construction, since the experimenter himself and many members of the staff continued to have the same experience throughout the series of experiments, nor was the effect diminished by practice. The general experience was of a solid, rigid body which, with change of horizon, tipped toward or away from the observer. The tilt was not simply forward or backward, but all observers declared that the "solid" was moving up and down as well. Some were able to describe the motion accurately as a curved path, downward and tilting forward, upward and tilting back. Now it is evident that a vertical cannot be seen to tilt in the line of regard, especially when there is no foreshortening. The observers were therefore asked to observe this movement when in the second position, slightly to one side of the line of motion, so that the verticals could be seen slightly side on. In this position also they reported the same experience. Asked to concentrate on the forward vertical edge, they all declared that it moved up and down, tilting at the same time. The only difference between reports for the two positions was that in the second position the vertical edge was now described as tilting "out and away from me" or "downward and moving off to the left," whereas in the first position it was described as "downward towards me" or "tilting back and moving up and away." Prolonged study of the phenomenon induced slight dizziness in some subjects and one subject felt what she described as "like being seasick" after watching this motion for about five minutes. Some subjects thought that the whole solid was moving up and down through space and one wondered whether he was himself moving! Prolonged examination does undoubtedly produce the impression that the object is floating on the surface of a liquid, like a box tossed about in the waves. The contradictory nature of the impressions is never very strongly to the fore, and even the writer was never able to experience the vertical as stationary with the most concentrated "analytic" study. The form was always named spontaneously as a "box" or a "safe," a "house" or a "cube." One subject, a machine draughtsman, termed it "a perspective cube."

The impression produced by change of vanishing points with horizon stationary was less sensational but equally compulsive. Subjects felt this difficult to describe. The "box" did not get smaller, but "sort of came together." It "had more perspective" but was still "a square box." All the transformations were accepted, the most highly acute perspective still being regarded as a "cube" or "box" when the projective included angle of the sides was as small as 60°! Lastly, the phenomena were most compelling at the higher rates of movement, though noticeable at low speeds so long as changes were perceptible.

Subjects found the comparisons rather difficult to make at the beginning of the experiment. Questioning revealed that their matches tended to be based on comparison of parts of the two figures. Requested to attempt a "snap" judgment of the two figures as wholes they began by declaring this "too difficult" and "feeling vague and unreliable" about their own estimates. With a little practice however, they accustomed themselves to carry out the instructions and all eventually declared that this method of estimation was as easy and pleasant as their original one.

With regard to matching of the varying solid with the series of projective comparison figures, Table I gives results for movement in the horizontal plane. Taking all four speeds together, each score is the mean of six trials (three in each direction), at each speed at four horizon levels. Under each condition the vanishing points varied and the constant errors may be seen to increase with decrease in perspective. Thus proceeding from V.P. 15 inches (greatest perspective) to 21

inches (least perspective), the error is greatest in the last condition, when the solid departs least from true orthogonality. Thus the subject may be said to tolerate a sharper perspective in the changing solid than in the stationary projection. Just as in experiments with tilted two-dimensional surfaces, the projective outline has to be more acute than that of the frontal-parallel comparison figure for the two to be judged similar in shape.

TABLE I

CONSTANT ERRORS AT THREE VANISHING POINT SETTINGS. HORIZONTAL TRAVERSE AT FOUR SPEEDS. EACH SCORE IS MEAN OF SIX TRIALS AT EACH SPEED, USING FOUR HORIZON LEVELS. N = 6912. SUBJECTS 24

	Subject	*Vanishing Point Settings* M^1	M^2	M^3
		15 *inches*	18 *inches*	21 *inches*
1	J.W.	0·6	1·5	2·2
2	R.S.	1·6	2·8	3·1
3	R.P.	0.7	1·0	1·5
4	J.N.	−1·1	1·3	1·5
5	J.M.	1·0	1·9	2·5
6	E.C.	0·7	2·6	4·2
7	S.D.	1·2	2·0	3·6
8	F.N.	0·6	2·4	2·6
9	C.J.	1·4	2·2	2·8
10	K.C.	0·6	1·8	2·4
11	A.C.	2·2	1·3	2·8
12	G.C.	1·2	1·3	2·4
13	R.B.	1·2	1·9	0·7
14	L.B.	2·0	2·8	3·1
15	V.C.	−0·5	1·0	2·0
16	J.C.	0·6	2·6	2·9
17	P.C.	1·6	2·4	3·2
18	B.C.	1·2	2·4	3·1
19	M.L.	1·4	2·4	3·4
20	R.A.	1·2	2·9	1·4
21	D.M.	0·7	2·4	1·6
22	A.M.	1·2	3·0	2·8
23	M.K.	0·4	2·3	1·8
24	S.S.	0·7	1·8	2·9
Mean =		0·97	2·1	2·5
σ =		1·25	0·79	1·62

Significance Test Between (1) M^1 and M^2: (2) between M^2 and M^3 using formula:—

$$t = \frac{MD}{\sqrt{\frac{(D - \bar{D})^2}{N(N-1)}}}$$

gives (1) 3·845
(2) 8·314
for N — 23 both are significant at 0·1 per cent. level.

Constant Error of M^1 by formula $t = \frac{\bar{X}\sqrt{N}}{\sigma}$ for a single mean; gives for $N - 1 = 23$, $t = 3·82$ significant at 0·1 per cent. level.

Table II gives analogous results, the matches being made with movement of horizon line (vertical) using comparison figures at four levels, 7, 9, 11 and 13 inches above zero level (horizontal). Each score is again the mean of six trials at three vanishing points repeated at four speeds. The picture shown by Table II is very different from that of Table I, for the constant error not only varies for each condition but also changes sign. Thus when the comparison figure is that of a solid with horizon lower than station point (position of observer) the error is positive, estimates being above parity, whereas when the horizon of the comparison figure is above the station point, the error becomes negative. The change in magnitude and sign of the error appears directly proportionate with change of horizon, the zero point coinciding almost exactly with the station point. This means that when the

comparison figure has a perspective orientation corresponding with the station point *which the subject actually occupies*, his estimate corresponds almost exactly with the comparison figure.

TABLE II

CONSTANT ERRORS AT FOUR HORIZON LINE SETTINGS. VERTICAL TRAVERSE AT FOUR SPEEDS. EACH SCORE IS MEAN OF SIX TRIALS AT EACH SPEED, USING THREE VANISHING POINTS. N = 6912. 24 SUBJECTS

Subject	*Horizontal Line Settings*			
	M_1	M_2	M_3	M_4
	7·0 *inches*	9·0 *inches*	11·0 *inches*	13·0 *inches*
J.W.	1·1	0·5	−0·5	−0·5
R.S.	1·3	0·15	−1·3	−2·4
R.P.	0·9	0·65	−0·3	−1·1
J.N.	2·1	1·0	−0·2	−2·0
J.M.	1·1	0·35	−0·5	−0·5
E.C.	2·6	1·1	−1·0	−2·1
S.D.	1·9	0·65	−0·8	−1·2
F.N.	1·8	1·0	−0·4	−1·9
C.J.	1·3	1·0	−1·0	−2·9
K.C.	0·7	0·5	−0·3	−1·1
A.C.	0·7	0·2	−0·6	−1·4
G.C.	1·1	0·75	−0·2	−1·0
R.B.	1·0	0·4	−0·1	−1·6
L.B.	0·8	0·25	−1·0	−1·7
V.C.	0·7	1·1	−0·4	−1·7
J.C.	0·7	0·7	−0·3	−1·5
P.C.	0·7	0·4	−0·6	−1·1
B.C.	1·5	0·8	0·3	−0·6
M.L.	1·6	0·4	−0·8	−1·4
R.A.	1·1	0·75	−0·1	−1·0
D.M.	1·7	1·0	−0·4	−1·2
A.M.	2·1	1·1	−0·1	−0·3
M.K.	0·7	0·6	−0·6	−2·0
S.S.	1·4	0·8	−0·2	−1·7
Mean =	1·27	0·67	−0·475	−1·41
σ =	0·52	0·31	0·36	0·98

It will be seen that the space errors and dispersion of scores are extremely small for all conditions, and similarly that all the errors are significant, both with respect to the stimulus value and with regard to each other. The mean range of scores was only 2·4 inches in the vertical and 1·6 inches in the horizontal axes. Significance of scores was estimated using Student's t and assuming that the compared means were correlate. (Significance is not given for the vertical condition since it is visible by inspection.)

VI

DISCUSSION

Before discussing the three questions raised earlier, certain points merit attention. The first is that no matter what the changes in real or apparent shape, the "solid" seems always to be perceived as an orthogonal object, even when the internal angles

become extremely acute. This might be interpreted by suggesting that the quality of solidity and orthogonality constitutes a characteristic, to use Michotte's phrase, a "cachet specifique," which once accepted enables all transformations to be held relatively invariant. The fact that the sole cue to difference between the solid and its projection (ignoring accommodation/convergence data, which will be discussed elsewhere) is the continuous self-movement of the former again suggests that deformation of the shape assists rather than interferes with the perceptual process, since the subject's tolerance of sharper perspective for the solid than is required for a stimulus match corresponds with the constancy phenomenon obtained in traditional shape-perception experiments.

The second point relates to the way the subject reacts to changes of horizon level. Here, the most important outcome is the change in sign of the constant error at the horizon level of the station point. Clearly, the subject cannot directly locate the true or the perspective horizon, since neither the horizontal ground plane nor the vanishing points are visible. At the same time, the results seem to suggest that he does "take into account" these factors. Perceptually, he cannot "know" what his own view-point is, but he reacts as if he did. The line of thought opened up by this result is extremely interesting in view of the way perception is generally assumed to operate. In this connection Piaget (1948, p. 229) states, "The purely perceptual point of view is always completely egocentric. This means that it is both unconscious and incomplete, distorting reality to the extent that it remains so . . . to discover one's own point of view is to relate it to that of others, to distinguish it from and co-ordinate it with them. To this task perception is quite unsuited . . . to do this requires a system of true mental operations" (*translated*). To the extent that the subject is able to "take into account" his own view-point on the basis of the cues provided by the changing "solid," it might therefore be concluded that his reaction is not purely and simply a perceptual one. On the contrary, it would suggest that acceptance of the perceived form as an orthogonal solid results from the intervention of developed mental operations of a schematic order which mediate the perceptual data in an abstract fashion. Thus the stimulus is reacted to in terms of schemata elaborated as reference frames for the abstract notion of orthogonal solid.

At first sight, there may appear a contradiction between results for the vertical and horizontal movements. In the vertical plane, the error is least when the comparison figure corresponds to the perspective appropriate to the subject's view-point (horizon level = station point). In the horizontal plane, the error is greatest when the comparison figure is closest to the perspective of a truly orthogonal solid. This contradiction however is more apparent than real, since in the first case there is no change of internal perspective for the "solid" but only a change of view-point. The error tends toward selection of a solid as viewed from the spatial position which the subject actually occupies. Secondly, in this first case the subject is in fact estimating the apparent tilt or relative orientation of the solid, whereas in the second case he is attempting to match changing perspective. The effect of the latter type of change is to make the more acute perspective figures appear differently proportioned and less like a "cube." Finally, the sharper included angles perceived at minimal vanishing points tend to encourage detail comparison of parts as against the "synthetic" comparison favoured by figures with vanishing points more widely spaced. At the same time, the constant decrease of error as the shape departs from true orthogonality calls for some further and more detailed study.

Bearing these points in mind, it should be evident that as regards the first question, as to whether reduction of ordinary perceptual objects to single

two-dimensional surfaces could be regarded as legitimate without qualification, the present findings are wholely negative and confirm the view put forward by Gibson (1951). Solid three-dimensional objects clearly possess perceptual properties not enjoyed by two-dimensional surfaces and are perceived as such in their own right. The impression of what Michotte has termed "substantiality," the quality of being rigid, indeformable, etc., appears to confer invariance or constancy in such a way that all changes of physical shape are referred to changes of orientation, resulting in compromise matches or constant errors of judgement.

This leads to the second question: between what is the compromise? or in Thouless's terms, to what is the phenomenal regression? Again, it is not simply a compromise between the visual character of the two stimulus objects. Rather is it a compromise between the visual data and the abstract notion or "schematic object" in terms of which these data are perceived. Thus it would seem that Thouless is correct in speaking of the "real" object as an "intuited" one, in the sense that the notion of object operates as a reference frame qualifying and mediating the perceptual data. The subject's reactions cannot be explained on the basis of most stable or most favoured gestalten, as suggested by Koffka, nor the simple outcome of interacting retinal gradients producing an objective perceptual estimate, as suggested by Gibson (1950). For on the basis of binocular retinal gradients of crossed disparity, or dynamic patterns possessing "prägnanz," the subject could hardly isolate a particular perspective and react to it without error simply because its horizon line and vanishing points (which he cannot see) correspond to his own spatial position and point of view. At the same time, an objection to the present interpretation could be offered, since the absence of error for one stimulus condition is only assumed to be due to the subject's "taking into account" his own view-point, on the basis of present results. For the present explanation to be more than suggestive, it would have to be shown that magnitude and sign of the constant error in the vertical plane varies with the subject's position and is directly related to it. As a check on this objection a subsidiary experiment was performed and its results are reported below.

Concluding the present discussion, it can be seen that as regards the third question, perspective plays only a minor part in perception of size or distance of an object. Changes of perspective corresponding to change of size or distance do not *alone* produce the impression of such changes, if only for the reason that (in the present case) perceived size remains unchanged. With change of horizon, perspective changes do not conflict with perceived size, since such changes do not involve radial movement of subject or object. With change of vanishing point however, perspective changes correspond to radial movement and hence conflict with direct perception of size. In this conflict, cues to size appear to be dominant and no change of distance is perceived.

If changes of distance perspective are not perceived as change of apparent distance, and physical deformation of the solid is not perceived as such, in what way are these changes accepted, how are the contradictions reconciled? It is suggested that the subject reacts to the object in a *representational* way. In other words, the idea of solid orthogonal object (as the subjects expressed it "a cube . . . a box . . . a safe") is part of an accepted representational convention. Any configuration capable of being related to or comprehended within this convention (whether diagrams, perspective drawings, animated cartoons, etc.) is perceived in terms of the abstract notions or concepts derived from the visual world which the convention formalises (*cf.* Gibson, 1954). Consequently, so long as the family of orthogonal solids which the moving framework can generate possess certain features

they can be granted a certain reality and accepted as such, whether or not they are in fact orthogonal or even possible solids, and the contradictory aspects of the situation are relegated to the background. In the case of perspective drawing Michotte (1948, p. 4) states, "Nous nous voyons forcés de souligner la distinction . . . entre le fait psychologue de la *croyance* à la realité d'un objet ou d'un événement, et le caractère intuitif de réalité qu'ils peuvent présenter."

TABLE III

CONSTANT ERRORS AT FOUR HORIZON LINE SETTINGS. VERTICAL TRAVERSE AT FOUR SPEEDS. EACH SCORE IS MEAN OF SIX TRIALS AT EACH SPEED, USING THREE VANISHING POINTS. UPPER TABLE: SUBJECTS 2 INCHES ABOVE ORIGINAL STATION POINT. LOWER TABLE: SUBJECTS 2 INCHES BELOW ORIGINAL STATION POINT. SIX SUBJECTS. N = 1728 FOR EACH POSITION.

$$\sigma = \sqrt{\frac{\Sigma X^2}{N - 1}} - \bar{X}^2$$

Subject	Comparison Figure Horizon Level			
	M_1	M_2	M_3	M_4
	7·0 *inches*	9·0 *inches*	11·0 *inches*	13·0 *inches*
2 inches Up. (A)				
R.S.	2·1	1·1	0·2	−1·1
E.C.	2·9	1·7	0·9	−1·2
K.C.	1·4	0·9	0·4	−0·6
G.C.	1·9	0·8	0·5	−0·7
B.C.	2·4	1·7	1·1	−0·5
D.M.	2·5	1·5	0·7	−0·7
Mean =	2·23	1·29	0·63	−0·8
σ =	1·05	0·35	0·74	0·46
2 inches Down. (B)				
R.S.	0·3	−0·4	−1·9	−2·7
E.C.	1·5	−0·2	−1·9	−2·9
K.C.	1·1	0·2	−1·0	−2·1
G.C.	0·7	−0·2	−0·9	−1·3
B.C.	0·9	−0·2	−0·4	−0·9
D.M.	1·1	−0·9	−1·0	−2·7
Mean =	0·9	−0·3	−1·18	−2·1
σ =	0·63	0·39	0·81	1·25

The same holds true of the contradictions implicit in perception of the solid "tipping over" with change of horizon. The fact that the vertical appears to remain upright whilst the laterals tilt without the solid changing shape as a whole, though logically a contradiction, is of no importance so long as the stimulus configuration possesses "object character." Here the contradiction is so flagrant as to be directly perceived. However, the impression of solid object, with all the attendant properties it confers, is so strong as to remain unaffected by those partial discrepancies. This point will be taken up again in the concluding discussion.

VII

Experiment III

With the aim of checking the results of Experiment II, the procedure described above was repeated with the subject occupying new positions. Six of the original subjects took part in this experiment and were seated in the same place as previously, but 2 inches higher (Position A) and 2 inches lower (Position B) than the original station point. Table III gives results for matches with the comparison figures used earlier. In the present experiment only the vertical plane was traversed, with vanishing points arranged at the previous three settings.

It will be seen that the earlier results are repeated and confirmed. The constant errors are again different in magnitude and sign, the zero point coinciding once more with the station point occupied by the subject. Differences between errors are again significant, whereas differences between each (for a given condition) and the one corresponding to the new position of the subject are not significant. Thus with the subject 2 inches above his previous position, his errors for the 7-inch condition correspond with his previous errors for the 9-inch condition, and vice versa. It is therefore evident that the subject takes into account his own view-point in matching the comparison figure and the "solid," since, when this changes with change of spatial position, his estimate as measured by the constant error changes also.

VIII

Conclusion

There remain a few points to be considered before closing. Firstly, with regard to the perception of solids, it has been pointed out that for a three-dimensional object to be perceived as such, it must be perceived as occupying depth; that is to say, the line of regard. This holds true for the perception of shape of a two-dimensional surface also, except when it lies frontal-parallel to the observer so that its projective shape corresponds to its euclidean shape. It has been suggested that the cues to perception of depth are not solely those derived from crossed disparity of binocular retinal stimulus patterns operating directly as interacting gradients; but that for depth to be perceived as a property of a three-dimensional solid, these data need to be mediated by some developed and abstracted schema or reference frame, activated by, but not originating in the retinal patterns themselves.

In attempting to justify "transactionist" theories of perception, Ittelson (1951) criticized Gibson's endeavour to account for perception and recognition of size and shape solely on the basis of retinal gradients without intervention of learning or similar processes. Ittelson pointed out that ascription of objective size and shape to perceived objects was, on Gibson's own showing, dependent upon acceptance of a subjective scale of evaluations by the subject himself. In other words, of certain "assumptions" about the object, not derived directly from the retinal pattern. In the present case, Gibson's (1950, p. 171) statement that the subject in a shape constancy experiment can "fairly successfully match the slanted object with an objectively equal but unslanted object *so long as he takes a naive attitude to what he sees*" (present writer's italics) involves the tacit admission that reference frames or schemata are operative in such perceptual processes, subsumed under the vague term "attitude." For surely the taking up of an attitude described as "naive" by Gibson or "synthetic" by Brunswik (1933) is no more than the adoption of a standpoint of unreflective acceptance of what is seen in terms of the familiar and experienced, as having the properties ascribed to physical objects. Hence the data are referred to developed schemata in terms of which they are perceived or reacted to. If this is the case, then "phenomenal regression" is certainly the elaboration of sensory stimuli in terms of what is "intuited" or grasped conceptually.

Finally, it is clear that the stimulus object employed here gave rise to peculiar and logically contradictory experiences. Also, the subject reacted to all types of perspective "solid" as being equally valid and it was pointed out that this could only occur on the basis of a representational convention. It is in order to point out that such behaviour corresponds roughly to that of an observer viewing a movie or television image. In this case the camera represents the station point, tracking and panning with resulting changes in viewpoint during the course of the production. The changing patterns of the image correspond therefore to changes of position (insofar as the scene appears "real" to the observer). Yet at the same time, the observer himself remains stationary. Nevertheless, he remains unaware of any contradiction between his total pattern of stimulation and that which he should receive were he, in fact, to occupy successively the different points in space from which the scene is taken. This suggests that even when he is emotionally involved, the subject's perception remains on the representational plane; his experience of the various points of view might be termed *vicarious*.

With the increase in perceptual "reality" such as is induced by 3 D or "Cinerama" techniques this may possibly be no longer the case. It might be suggested that some of the curiously unsettling experiences created by films of this type derive from intensification of conflict between contradictory sets of cues, one set belonging to the "camera eye" occupying one or a number of positions, and the other to the "viewer's eye" occupying another. That is to say, the perceived scene can no longer be accepted on a wholly representational plane. This effect is, in practice, minimized by the limitations at present imposed by 3 D and "Cinerama" technique on camera movement.

If such a suggested explanation is correct these phenomena in a different, though cognate, field demonstrate once more the abstract character of perception. Such perception presumes the intervention of learning, the acquisition of reference frames and thereby the activity of conceptual or intellectual processes. At the same time, such speculations require further investigation, both as regards the role of convergence/accommodation cues and binocular vision, and also in the wider field of the relation between perception of objects and perception of moving pictures.

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A NOTE ON SUMMATION TIME OF THE EYE INDICATED BY SIGNAL/NOISE DISCRIMINATION

BY

R. L. GREGORY

From the Department of Psychology, University of Cambridge

Recent work has shown that the photochemical theory of dark adaptation is inadequate. A number of considerations, such as the effect of vitamin A deficiency upon scotopic sensitivity suggest that the mechanism of adaptation is at least partly photochemical, but that it is not entirely so is now established apparently beyond doubt. Rose (1948), and other writers, have found that changes in concentration of rhodopsin are probably less during dark adaptation than corresponding changes in sensitivity: this has been directly demonstrated for the living human eye with a technique due to Rushton (1952). The results are reported by Campbell and Rushton (1954), and Rushton and Campbell (1954). It is found that only a small part of adaptation can be photochemical, the rest appears to be neural.

Perhaps a number of neural mechanisms might be suggested: only one will be considered here, namely, the possibility that the summation time of the eye increases with dark adaptation. By "summation time" or "storage time," is meant here the interval over which the effect of the stimulus energy is integrated. It is well known that the response time of the eye increases with dark adaptation, as may readily be demonstrated with Pulfrich's pendulum (Banister, 1932) (at least for cone adaptation), as shown by Lythgoe (1938). But although to be expected, this increased delay does not show that storage time increases with dark adaptation. The following observations were made in an attempt to provide more direct evidence.

Now it appears to be useful, in many respects, to regard the eye as an information source working into a noisy channel. It is generally true of information systems with storage that the signal-to-noise ratio is related to the storage time (see Bell, 1953, p. 50). Where M is the number of repetitions of the signal, S the signal amplitude and N the noise amplitude, noise increases by $(MN^2)^{\frac{1}{2}}$, while S increases linearly with M. Thus S/N improves by $M^{\frac{1}{2}}S/N$. It therefore appears that the ability of the eye to discriminate a signal masked by random "noise" should give a measure of summation time, and this will be independent of retinal delay. The following experiment was designed to test this idea.

The visual display used was an explicit signal/noise arrangement. A random noise source (a thyratron placed within a magnetic field) was mixed with the output from a 250 c/s. sine wave generator; the resultant noise-modulated sine wave was fed to a C.R.O. whose screen provided the visual display. The C.R.O.s time-base was set to 50 c/s. and locked to the signal generator. The resulting stationary chain of five sine waves, more or less masked by noise, was viewed by the subject. The C.R.O. tube had a short persistence screen with a green trace.

The expected effect of increase in summation time upon S/N is shown by the series of photographs. Each shows the same value of S/N set up on the C.R.O., but for a longer exposure time in each case. It is to be expected that, if the summation time of the eye increases with dark-adaptation, discrimination of the signal against the noise should with this arrangement improve similarly.

An alternative way of expressing the improvement with increase in summation time with this arrangement is to regard the signal as the statistical Mean of the noise. Increase in the sample size with time will increase the accuracy of the position of the Mean, by $\sigma\sqrt{n}$, where σ is Standard Deviation and n the sample size.

The display was viewed monocularly through a calibrated variable-density (crossed Polaroids) filter. The signal and noise amplitudes were controlled with switched attenuators, and some control over the noise spectrum was provided by parallel-T filters. Ambient light could be provided by a slide projector illuminating a white screen surrounding the C.R.O. tube, which was itself shielded from the light with an opaque disc placed between glass in the projector's slide holder.

It was found (by switching the signal "on" or "off" and asking the subject to report its presence or absence) that signal-to-noise discrimination improved by a factor of 2 when intensity was reduced by 2·5 log. units, in the absence of ambient light. That this is not due simply to acuity loss is indicated by the fact that the effect is not appreciable for the (static) photographs; also it does not occur in the presence of ambient light, although this should impair visual acuity in addition to producing general light adaptation.

At low intensities rapid movement of the "noise" was entirely absent, the display appearing as a steady blue. Rapid movement of the "noise" was, however, clearly evident in the presence of ambient light even when the trace intensity was adjusted close to differential threshold. Just such a reduction in temporal acuity would be expected if summation time increases with dark adaptation.

It would perhaps be unwise to attribute more than a small part of the increase of the sensitivity of the eye during adaptation to increase in storage time. Its probable importance might be considered in terms of (*a*) increase in the effective number of quanta available to initiate a response, and (*b*) to improvement in the signal/noise ratio of the visual system itself, which we should expect for the reason give above.

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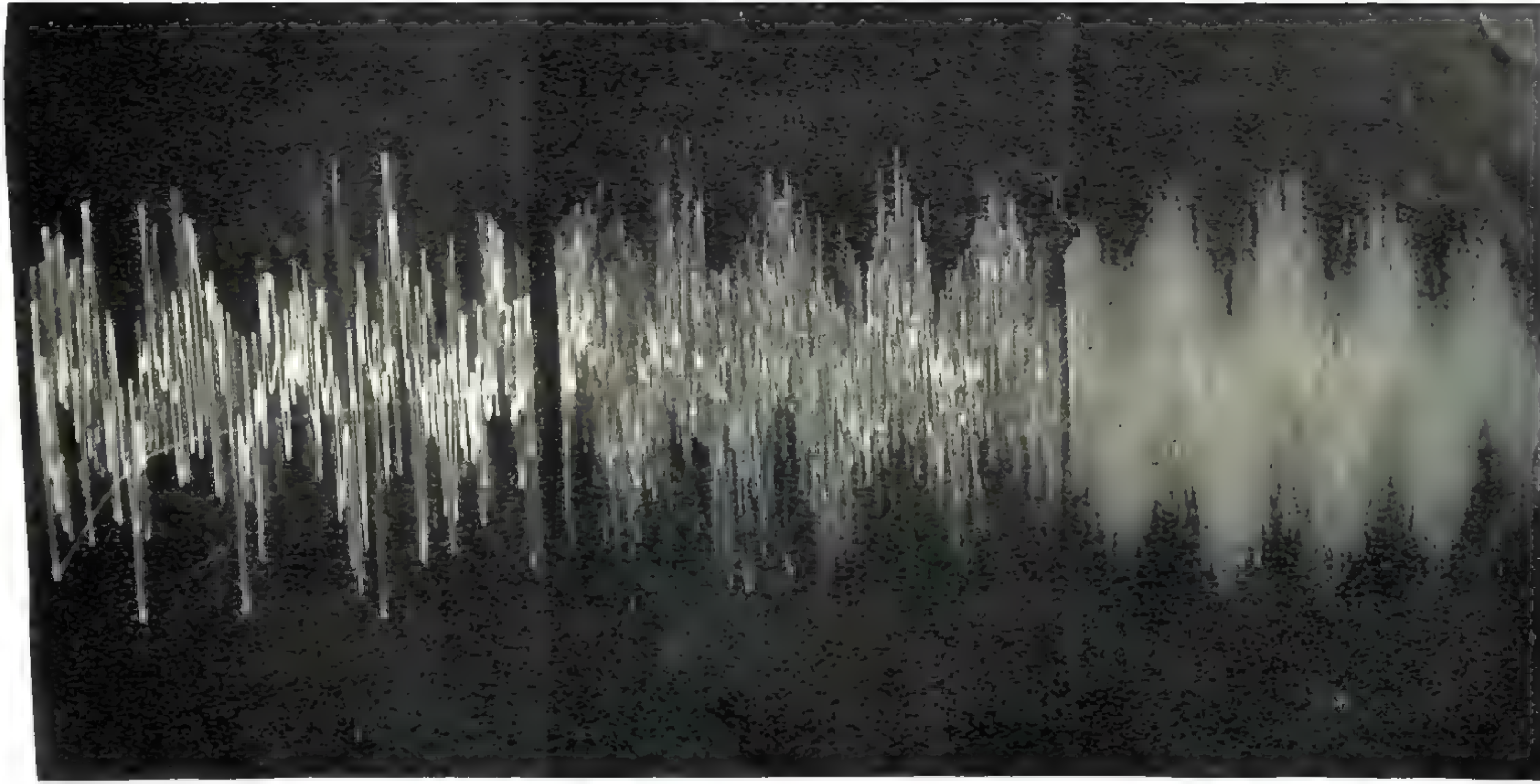
FIGURE 1



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VISUAL ILLUSION AND FIGURAL AFTER-EFFECT, WITH AND WITHOUT FIXATION

BY

ARTHUR SUMMERFIELD AND KENNETH M. MILLER*

From the Department of Psychology, University College, London

An experiment, which was carried out with forty observers to investigate the effects of two conditions of observation, fixation and non-fixation, on the relative magnitudes of figural after-effect and immediate illusion is described. The combined inspection figure and test figure used in investigating after-effect displacements formed the figure used in investigating illusory displacements. An account is given of the apparatus used which enabled quantitative measurements of the displacements to be made by a production method. A second experiment on the magnitude of illusion and after-effect occurring with a slightly different figure and with fixation the only condition of observation is also briefly referred to.

The results of the experiments indicate (*a*) that illusory displacements exceed after-effect displacements, contrary to Köhler and Wallach's (1944) non-quantitative observation, (*b*) that virtually no after-effect occurs in the absence of fixation as observation condition, (*c*) that while fixation is necessary to the production of after-effects, observation without fixation favours a significantly larger illusory effect.

It is concluded that, in spite of the similarities between after-effect and illusory displacements, differing mechanism must be postulated as underlying them, and, therefore, that neither Köhler and Wallach's theory nor Osgood and Heyer's (1951) theory of figural after-effect is capable of immediate extension to cover illusions.

In the final chapter of their monograph on "Figural after-effects," Köhler and Wallach (1944, p. 352) observe that "Some familiar illusions are so similar to distortions which have here been described that we must ask ourselves whether this similarity is only accidental." The similarity has a two-fold basis. First, when an inspection figure, I-Figure, whose after-effect is demonstrated by distorted perception of an associated test figure, T-Figure, is combined into a single diagram with such a test figure the result in many, though not all, cases is a diagram well known as a geometrical illusion. Secondly, where in these cases a part of the after-effect consists of an apparent displacement of parts of the test figure such displacements are of the kind reported as characteristic of the illusion. Notable cases of this kind appearing among the examples given by Köhler and Wallach are the Delboef and Ponzo illusions.

The superficial similarities are striking and it would evidently be an advance if a single principle of explanation could be formulated. As Köhler and Wallach (p. 353) observe however "It is one thing to grant that a formal analogy exists;

*Now at the University of Tasmania, Hobart, Australia

but as Gibson rightly remarked it is quite another thing to maintain that the figural after-effects and the immediate illusions are functionally of the same nature." These authors themselves cautiously explore the hypothesis that *immediate self-satiation* may be responsible and offer some tentative evidence. This hypothesis represents an extension of their theory of figural after-effects which itself has elsewhere received other applications and criticisms.

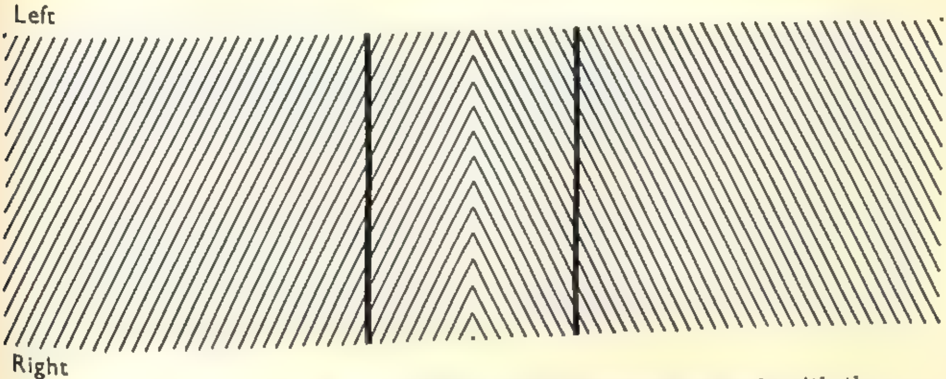
An aspect of the problem which is not explicitly examined by Köhler and Wallach is the part played by fixation, yet this would seem very relevant to the comparison of after-effects and illusions. Fixation of a definite mark does not appear a prerequisite for the observation of illusions. Point fixation was however invariably used in Köhler and Wallach's studies. The whole development of their satiation theory with its emphasis on cumulative changes occurring at particular cortical loci requires the pattern of retinal stimulation to be relatively unchanging. The same requirement is implied by Osgood and Heyer's (1951) alternative theory.

An examination of the reports of studies involving the after-effect of one configuration on another does not seem to remove the need for a condition of observation approaching fixation if any kind of after-effect is to be reported, though, as Smith (1948) has pointed out, Gibson (1933) originally encountered figural after-effects as the general after-effect of having worn distorting spectacles for a period of time, fixation not being involved. Gibson (1937*a*), referring to his earlier studies of the after-effect of curved and bent lines, reports negative after-effects as occurring: "If a subject is required either to fixate or merely to look at a curved line the quality of curvature decreases in the course of time and *in that particular region of the subjective visual field* [Gibson's italics] a straight line appears bent the other way. The same is true of a bent line if the subject's gaze is confined to the section of the line where the bend appears." In his later studies (1937*b*, *c*) subjects were required to fixate. Vernon (1934) in her study of the perception of inclined lines required her subjects when shown the figure to be inspected "to look at it steadily (though without definite fixation)." Following Köhler and Wallach's procedure fixation has invariably been required by the numerous subsequent investigators.

The need for, the part played by and the effects of, fixation in this kind of observation appear obscure. The experiment to be described was undertaken to investigate the relative magnitudes of the displacements (distortions) occurring either as after-effects or as immediate illusions when fixation was and was not a condition of observation in each case. The distinction between the fixation and non-fixation conditions was made (*a*) in terms of the instructions given to the observers and (*b*) by the provision of a fixation mark when the instructions were for fixation and its omission when the instructions were for observation without fixation. The figure used for investigating displacements occurring as an immediate illusion consisted of the combined I-Figure and T-Figure used in investigating displacements occurring as after-effects. The choice of figure was influenced by the requirements that the effects were to be measured and by the two conditions of observation to be used. The figure illustrated was chosen, Figure 1. It belongs to the class of illusions involving lines intersecting at small angles, of which the most compelling are the twisted-cord illusions of Fraser (1906), though these are not well suited to quantitative investigation. The illusion producing properties of the "herring-bone" field have been investigated in other contexts, e.g. Orbison (1939) and Berliner and Berliner (1948). The figure chosen readily permits realization as an adjustable apparatus with which production methods can be used. It is relatively complex and does not present any one inherent fixation point, as might be the case

with the apex of the single angle in the Ponzo figure, which can be regarded as a simpler version.

FIGURE 1



The figure used for investigating illusory displacements, and, with the "herring-bone" field separated as I-Figure and the "parallel" lines as T-Figure, for investigating after-effect displacements also.

Method

Experimental design.

The immediate and after-effect conditions and the two observation conditions (with and without fixation) were combined in a 2 × 2 factorial design. The four treatments were:—

AF—after-effect with fixation;
AO—after-effect no fixation;
IF—immediate effect with fixation;
IO—immediate effect no fixation.

Four independent groups of ten observers each were used, enabling the analysis of variance to be applied to the results. Since a production method was used and the *equivalent parallel position* could be approached from an initial right-hand divergence or convergence of the intersecting lines whose convergence was variable (see Figs. 1 and 2), the 4 groups were each further divided into 2 sub-groups of 5; the first trial for one sub-group in each case began from initial divergence and for the other from initial convergence. Initial convergent and divergent settings were subsequently alternated on successive trials for all groups. Adjustments were made by the experimenter, what Guilford (1936, p. 134) has termed the psychophysical method of equivalents being used. This method was preferred to that of having adjustments carried out by the observer since it enabled the duration of the adjustment process to be controlled by the experimenter.

Subjects.

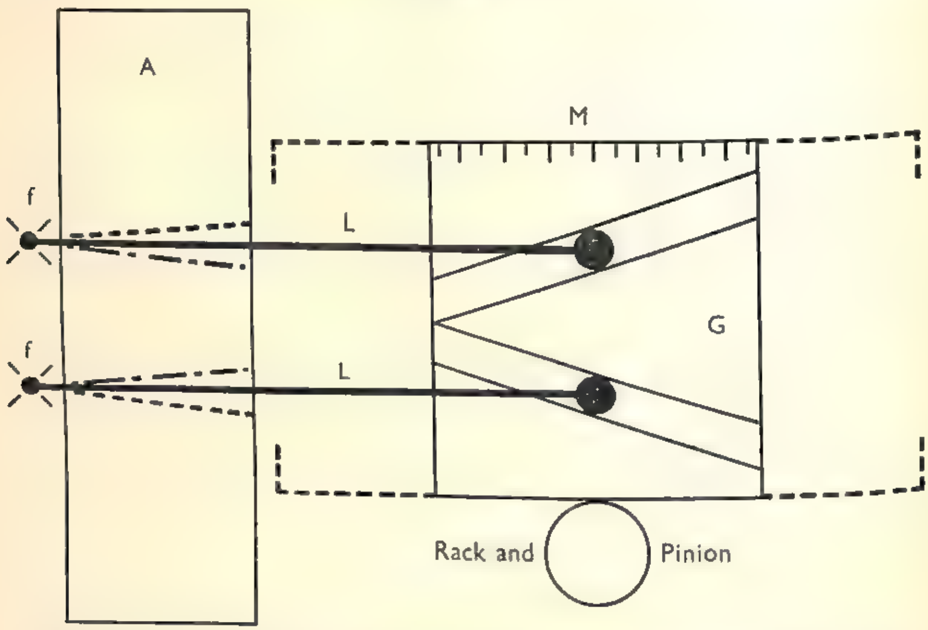
The observers were forty first-year students of psychology with experience of visual experiments involving fixation but unacquainted with the nature and purpose of the experiment. A formal randomization process was used to allocate them to the eight groups and sub-groups.

Apparatus.

A schematic diagram of the apparatus, as viewed from the observer's position, is shown in Figure 2. The rectangular aperture A, 9 in. × 3 in. was centrally placed in a large matt black screen (not shown) which concealed the whole of the controlling mechanism from the observer. The "intersecting lines," L, were of one-sixteenth in. rigid rod, painted matt white, and pivoted symmetrically with 2 in. between centres at the left hand edge of the aperture. The free end of each rod ran in one arm of the V-slot in the sliding guide plate (G). The length (l) of each of the rods (L) between free and pivot centre was 18·8 cm. The bearings on the free ends of the rods were a sliding fit in the V-slot, and the guide plate (G) was itself a sliding fit in its own guides so that the system was free from backlash. The guide plate carried a millimetre scale (M),

reading zero when the rods were objectively parallel. The position of the guide plate was adjustable left and right via a rack and pinion operable by the experimenter. The angle of the V-slot was 2θ, with the half-angle $\theta = 28°$, giving on the horizontal scale (M) a magnification by 1·88 : 1 of vertical displacements of the free end of each rod.

FIGURE 2



Schematic diagram of the apparatus. *A*—aperture in screen; *G*—sliding guide plate with V-slot; *LL*—"intersecting lines" of adjustable angle, pivoted at *ff*; *M*—millimetre scale carried by *G*.

Cards, with or without a fixation mark, and whether plain matt black or carrying the herring-bone design ruled in white on a matt black ground, could be inserted in slots so that they filled the aperture (A), either immediately in front of or immediately behind the rods (L). The half-angle of the herring-bone design was *arc tan* 1/2 = 26° 34′. The white lines were 0·3 mm. thick and the black spaces 3·5 mm. For the fixation conditions a circular white spot, diameter 2 mm., was placed centrally on the card. The rods when parallel were equidistant from the line formed by the angles of the herring-bone and the fixation mark when included lay on the mid-point of this line.

The observers viewed the aperture binocularly through a circular hole in a second screen carrying a head rest, used whether judgements under fixation or nonfixation were being made. This screen also carried the lamps used to give uniform illumination of the aperture and shielded the observer from direct illumination by them. The experiment was carried out in a darkroom so that illumination could be kept constant, but not under conditions of dim illumination. Observers were so accommodated in the apparatus that the centre line of the aperture (and therefore the fixation mark when used) was at eye-level, and at a distance of 100 cm.

Procedure.

For groups AF and AO each trial consisted of inspection of the I-Figure, i.e. the herring-bone field, with or without fixation for 2 minutes. The I-Figure was then withdrawn from its position in front of the adjustable rods which now appeared alone against a plain matt black ground and formed the T-Figure. A fixation mark appeared in both I- and T-Figures for the AF group and was absent from both for the AO group. The rods, having been previously set to an initially convergent or divergent position were immediately adjusted until the observer reported that they were apparently parallel (the *equivalent parallel position*). For Groups IF and IO the complete figure

of rods against herring-bone field was presented and each trial consisted of immediate adjustment to the equivalent parallel position as reported by the observer. For all groups a screen was placed in front of the aperture between trials. The adjustment time was naturally slightly variable occupying between 10 and 15 seconds. Two minutes elapsed between trials and ten trials were made with each observer.

Instructions.

The following instructions were given to subjects of the various groups.

Group AF: "Behind this screen [the screen covering the aperture between trials] is a diagram in the centre of which is a white spot. When I remove the screen look as fixedly as you can at the spot. At the end of two minutes I shall change the diagram and you will see another also with a white spot. Again look fixedly at the spot. Across this second diagram there will be two lines. I shall want you to judge when these lines look parallel to one another. Say "*now*" as soon as the lines look to you to be parallel. Remember you must concentrate on looking fixedly at the spot in both diagrams all the time."

Group AO: "Behind this screen is a diagram with a line across the centre [the line formed by the angles of the herring-bone]. When I remove the screen look steadily at the diagram. At the end of two minutes I shall change the diagram and you will see another. Across this second diagram there will be two lines. I shall want you to judge when these lines look parallel to one another. Say "*now*" as soon as the lines look to you to be parallel."

Group IF: "Behind this screen is a diagram in the centre of which is a white spot. When I remove the screen look as fixedly as you can at the spot. Across the diagram there are two lines. I want you to judge when these lines look parallel to one another. Say "*now*" as soon as the lines look to you to be parallel. Remember you must concentrate on looking fixedly at the spot all the time."

Group IO: "Behind this screen is a diagram with a line across the centre. Across the diagram there are two other lines. When I remove the screen I want you to judge when these two lines look parallel to one another. Look steadily at the diagram and say "*now*" as soon as the lines look to you to be parallel."

For groups AF and AO the instructions for the judgment to be made and the response to be given were repeated as the T-Figure was exposed, and the instruction to look fixedly at the spot was repeated to groups AF and IF as the screen was being removed each time.

Measure of the dependent variable.

The measure recorded for each trial was the "error," i.e. the deviation in millimeters from zero on the scale (M) which corresponded to the equivalent parallel position as determined on that trial. Since the illusion consists in apparent right hand *convergence* of the crossing lines when these are objectively parallel (see Fig. 1), the convention has been adopted of designating as *positive* measures corresponding to actual right hand *divergence* of the crossing lines, such errors indicating operation of the illusion.

From the dimensions shown in the description of the apparatus given above, it will be evident that a displacement of $\pm x$ millimeters on the horizontal scale corresponds to an angular displacement of the rods from the parallel position such that, if ϕ is the convergence angle of the rods, then

$$\pm \phi = 2 \text{ arc tan } (\pm x \cot \theta / l) = 2 \text{ arc tan } (\pm x/100)$$

ϕ is zero when the rods are parallel and the convention for the sign of x may be extended to ϕ so that x and ϕ are designated as positive for right hand divergence. The above formula does not make allowance for the fact that the free ends of the rods move on arcs of circles rather than strictly vertically, but $x \cot \theta$ is small compared with l and the correction is negligible. For the same reason, the relation for small angles that

$$\tan \phi \simeq \phi \text{ radians}$$

holds, so that ϕ can be regarded as effectively a simple linear function of x. While angular measures have a greater generality, the analysis described in the next section has nevertheless been based directly on the scale measures in virtue of this relation, but no difficulty arises over the conversion; multiplication of values of x in millimetres by 1·146 gives values of ϕ in degrees of arc, and the transformation may be applied to mean and SD values.

Results

The data were treated as follows. The value of the equivalent parallel position over all ten trials was computed for each observer and the analysis of variance applied to these measures. The factors entering the three-way classification analysis were Effect (after-effect or immediate illusory effect), Observation Condition (fixation or non-fixation) and Initial Setting (convergent or divergent setting on the *first* trial); the analysis yielded four interaction terms.

The outcome of the analysis is presented in Table I. It is dominated by the highly significant difference between "effects." Apart from this there is a substantial contribution to the total variance from the interaction of "effects" and "observation conditions," all other terms being non-significant. The mean values for the main effects are given in Table II. All are positive, indicating distortions

TABLE I

Analysis of Variance—Applied to Measure of Equivalent Parallel Position, as Determined from 10 Responses. (N = 40)

Source	*Deviances*	*df*	*F*
Main Effects			
Effects (A.E *v.* I.I)	558·3826	1	107.67*
Observation Conditions	4·1281	1	
Initial Settings	6·4401	1	1·24†
Interactions			
Effects × Observation Conditions	40·3005	1	7·77‡
Effects × Initial Settings	3·5105	1	
Initial Settings × Observation Conditions	1·0726	1	
Effects × Observation Conditions × Initial Settings	1·0401	1	
Residual W/Groups§	165·9480	32	
Total	780·8225	39	

* Probability less than 0·001.
† Probability greater than 0·200.
‡ Probability less than 0·010.
§The residual *variance* is equal to 5·1859.

TABLE II

Analysis of Variance—Mean Values* in Millimetres for Each Level of the Main Effects

Effect		*Observation Condition*		*Initial Setting*	
Illusion A-Effect	+9·1 +1·6	Non-fixation Fixation	+5·7 +5·0	Divergent Convergent	+5·8 +4·9
Differences	7·5†		0·7		0·9

Residual Variation: $SD = 2{\cdot}3$ millimetres

* All the mean values differ from zero beyond the 1 per cent. level of significance.
† Significant beyond the 0·1 per cent. level.

from zero, the objective parallel position, in the direction to be expected. The significant difference between "effects" is seen to arise because the immediate illusory effect is much greater than the after-effect, irrespective of observation condition.

If attention is confined to the relative magnitudes of the distortions occurring when fixation is the observation condition for both after-effect and illusion, the difference is smaller but remains highly significant. ($P < 0{\cdot}001$). The mean values are given in the first line of Table III. This finding was also borne out by the result

TABLE III

INTERACTION BETWEEN EFFECTS AND OBSERVATION CONDITIONS—MEAN VALUES*

Condition	*Immediate Illusion*	*After-Effect*	*Overall Means*
Fixation	+ 7·8 mm.	+2·3 mm.	+5·0 mm.
Non-fixation	+10·4	+0·9	+5·7
Overall Means	+ 9·2 mm.	+1·6 mm.	+5·4 mm.

* With the exception of the mean value for after-effect under non-fixation ($P > 0{\cdot}2$) all of the means differ significantly from zero beyond the 1 per cent. level.

of a second experiment which was carried out in precisely the same manner as that described except that fixation was the observation condition for all observers ($N = 32$, 16 observers per group), and the black/white relations of the diagram were reversed. The mean values are given in Table IV.

TABLE IV

COMPARISON OF ILLUSION AND AFTER-EFFECT UNDER FIXATION FOR BLACK ON WHITE CONFIGURATION ($N = 32$)

Effect	*Mean Value*
Illusion	+7·1
After-effect	+2·6
Difference	4·5 mm.*
SD	2·3 mm.

* Difference significant beyond the 0·1 per cent. level.

Returning to Table I, it is to be noted that the analysis reveals no simple effect of observation condition, there being no overall significant difference between fixation and non-fixation. The significant interaction between "effects" and "observation condition," however, indicates an influence of these two factors in combination over and above the "effects" difference. The four mean values involved have been given in Table III, above, and the nature of the interaction is revealed as being that fixation favours greater after-effect displacement while non-fixation

favours greater illusory displacement. All the mean values are positive, but that for after-effect under non-fixation is not significantly different from zero.

The raw data were further examined for evidence of cumulative effects over the ten trials made with each observer. The average value of the measures for the first two trials were compared with the averaged value for the last two trials. (The first two trials and the last two trials were taken, rather than the first and last, to exclude variation arising from initial setting.) In no group was this difference found to be significant. The mean values and *SD*'s are given in Table V. The

TABLE V

Mean Values and Differences of First and Last Two Trials for Each Group

(n = 10 per group)

Mean Value (in mm.)	Group AF	AO	IF	IO
First two trials ..	+2·7	+1·3	+10·3	+10·9
Last two trials ..	+3·9	+1·6	+ 8·5	+10·7
Differences ..	−1·2	−0·3	+ 1·8	+ 0·2
SD_{diff}	2·0	2·5	3·7	2·6

mean differences are not only not significant but are absolutely small, indicating the appreciable stability of the results from trial to trial.

An incidental result of the experiment emerging from the reports of observers in the *AF* group and noted by both experimenters was the finding of phenomena attendant on long fixation closely analogous to those reported by Marks (1949).

Discussion

The implications of the experiment are first that, with the figure used and with the given method of investigation, there is a differential effect of fixation *versus* non-fixation on perception of the illusion compared with perception of the after-effect. This inference follows from the finding of an appreciable interaction between *effect* (of illusion and figural after-effect) and *observation condition* (fixation or non-fixation) in the absence of any straightforward overall difference between fixation and non-fixation. The after-effect resulting when observers were not required to fixate during observation of I-Figure or T-Figure was not found to differ from zero. This result was to be expected both on the empirical ground that the procedure used by previous investigators has involved fixation and on the basis of either of the theories offered in explanation of figural after-effects, the satiation theory of Köhler and Wallach (1944) or Osgood and Heyer's (1951) extension to figural after-effects of Marshall and Talbot's (1942) theory of visual acuity. That the illusion should be significantly greater in the absence of fixation is not however predictable on the hypothesis that either theory of figural after-effect has application to immediate illusions also. This hypothesis has therefore to be rejected.

In the second place the results of both experiments to which reference has been made do not bear out the statement which Köhler and Wallach (1944, p. 353) make about the Ponzo illusion, but which they do not support with quantitative evidence, that "this immediate illusion is less conspicuous than the figural after-effect." The

figure we have used is, as has been noted, closely related to the Ponzo figure; when fixation was the condition of observation for both illusion and after-effect our finding is that illusory displacements are the larger.

It remains necessary to consider a possible qualification to these conclusions. As is reported above, some 10 to 15 seconds elapsed between the end of fixation of the I-Figure and conclusion of the adjustment process which yielded the measure of the equivalent parallel position on each trial. While there are a number of reports of casual observations on the duration of figural after-effects, the only quantitative data on their rate of development and rate of decay seem to be those of Hammer (1949). The figure she used for her investigation did not involve intersecting lines, but was an after-effect of the kind involving relations between areas of differing brightness. So far, therefore, as her results can be considered in relation to those of the present experiment, their implication is that a small but measurable decrement in the after-effect is likely to have occurred before the adjustment process was complete. If it occurred this decrement would have had the effect of magnifying the difference between after-effect and illusion. On the other hand Hammer's data indicate that while the after-effect producing condition should be fully developed after fixation of the I-Figure for 2 minutes, only an incomplete development should have occurred in 15 seconds; in the present experiments the total observation time when illusory displacements were measured was of this order, being simply the time to complete the adjustment. In fact, when Hammer's curves for development and decay are superimposed they suggest that the effects should be virtually identical for a time of 10 to 15 seconds. The implication therefore remains that the substantial difference found in the present experiments is not attributable to temporal effects of this kind.

The two results of the experiments, (*i*) the differential effects of fixation and non-fixation and (*ii*) the finding of larger illusory displacements than after-effect displacements when fixation was the condition of observation for both, together suggest that different or at least differing mechanisms must be presumed to underlie figural after-effects and immediate illusions. The cautious approach to this question taken by Gibson and by Köhler and Wallach appears to be fully justified.

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FATIGUE DURING PROLONGED PERFORMANCE ON A SIMPLE COMPENSATORY TRACKING TASK

BY

G. J. SIDDALL and D. M. ANDERSON

From the Ministry of Supply Clothing and Equipment Physiological Research Establishment, Farnborough

Two basic activities are involved in radar operating, watchkeeping and target tracking. Deterioration of watchkeeping performance has been explained by a hypothesis which relates absence of sensory variation and failure to attend to one part of the display continuously (Broadbent, 1951). This paper describes a preliminary experiment to examine deterioration on a simple compensatory tracking task.

Twenty-one subjects were each tested for an uninterrupted period of two hours. They were required to keep a target correctly aligned by cranking a handwheel at a constant speed, and it was expected that failures to attend to the display would result in target deviations. Analysis of the results showed that both number of errors and mean duration of errors increased significantly in consecutive half-hour periods, and that there were large individual differences in performance.

I

Introduction

In general, radar operating may consist of either or both of two basic activities. These are (i) keeping watch for targets and reporting their presence when they appear, and (ii) tracking the targets.

A considerable amount of work has been reported on the effects of fatigue on the kind of perceptual activity involved in watch-keeping. Mackworth (1950) has drawn attention to the increase in the number of relevant signals that are missed as the period of watch continues beyond 30 minutes, and Broadbent (1951) has interpreted this type of performance deterioration in terms of a failure to attend to one part of the surroundings continuously, drawing a parallel between such lapses and the "blocks" preventing response as reported by Bills (1931) in a series of sensory discrimination tasks. Fraser (1951) had previously found that response to an unchanging part of the stimulus field showed an uneven distribution in time as the duration of the activity increased, and from this developed a technique of testing for fatigue using as a measure the variance of the subject's estimations about his own mean (Fraser, 1952).

A further principle is that any factor causing the attention to be divided over different parts of the display tends to reduce the incidence of missed signals and may reinstate performance at a previous high level. This applies equally to the presence of the experimenter in the same room as the subject (Fraser, 1953) and to the presence of knowledge of results in the situation (Mackworth, 1950).

Hence the two most important features of vigilance or watch-keeping tasks are the presentation of discrete relevant signals at infrequent intervals of time, and the relative absence of discontinuity in the stimulus field. Absence of discontinuity as understood in the present context is, of course, a milder case of perceptual deprivation than that studied by Bexton, Heron and Scott (1954). These authors isolated subjects for periods of 24 hours or more and found deterioration in cognitive functioning and the development of psychopathological symptoms similar in some ways to the symptoms of fatigue in the series of synthetic flying experiments reported by Davis (1948).

The tasks used by Bills, and the five-choice serial-reaction test used by Broadbent, were characterised by only a relative absence of discontinuity in the stimulus field, and relevant signals were presented at a fairly high rate. These tasks might be termed "consecutive" to distinguish them from the more truly continuous tasks, such as tracking, where the timing and nature of the next signal in the series depend upon the previous response.

The experiment reported here represents a preliminary attempt to analyse deterioration in performance on a simple continuous task—compensatory tracking of a target which drifted off centre in a constant direction at a constant rate. No difficulty was experienced in performing error-free runs of at least 10 minutes' duration and it was assumed, therefore, that any subsequent errors would be due to inattention to the display. Since there were no discrete signals it was considered that there was greater uniformity of the display in space and time, i.e. there was less discontinuity, and that the conditions were closer to the perceptual isolation of Bexton and his co-workers, than the previous types of task outlined above.

II

Method

Apparatus.

The synthetic display used was similar to that in certain types of radar set. The subject was required to keep a target echo constantly aligned in the corner of a step in the time base.

The display consisted of two pieces of perspex; one fixed, representing the time base, and the other moving, representing the target echo. They were both suitably engraved and edge-lit. When superimposed and viewed from the front, they appeared in one plane and gave a good impression of a C.R.T. display.

The moving part of the display was connected to the output shaft of a differential. Thus its motion was the algebraic sum of motion fed in by the subject *via* the handwheel connected to one input shaft, and a constant speed motor which fed the other input shaft.

The handweel was 6¾ in. diameter and had a moment of inertia of 6·2 lbs. in.^{2} With the target motor running the handwheel had to be cranked at a constant speed of 75 r.p.m. to keep the target stationary. When the motor was not running, 25 revolutions of the handwheel moved the target through three inches on the display.

Errors to the right and left were recorded on post-office counters driven by a vibrator at 10 impulses per second, which was located in a room some distance from the main apparatus. Switches on the output shaft from the differential closed as soon as the error became greater than 1 mm. movement to right or left of the corner of the step. The experimenters recorded continuously these errors as they were made, noting the length of error and the time.

Procedure.

Twenty-one subjects (age range 18 to 33) were tested for an uninterrupted period of two hours. Each was first given five minutes' practice to become acquainted with the apparatus. Red error-indicator lights were switched on during the practice period and the extent of the zone of accuracy was learnt from them. This was followed by short runs without the error lights during which the subject was urged to track as accurately as possible. Immediately he had accomplished an error-free run of four minutes, practice was ended and after a short rest his watch and cigarettes were taken away and the two-hour test was started.

III

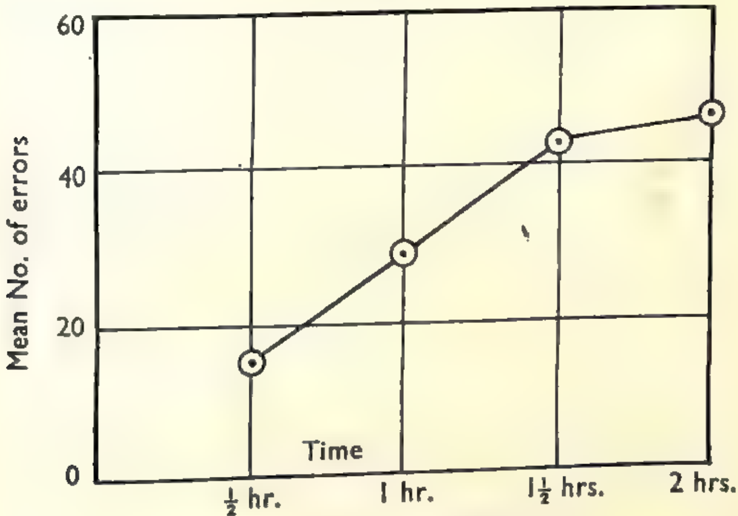
Results

Incidence of Errors.

The total number of errors (regardless of sign), for each subject ranged between 15 and 324 with a mean of 131·8.

In order that the progressive effect of fatigue could be determined, the full two-hour period was split into four half-hourly periods, and the mean number of errors for all subjects was plotted against time (Fig. 1).

FIGURE 1



Mean number of errors against time.

The data conformed to a Poisson distribution and to make the error variance homogeneous, a square-root transformation was performed. Analysis of variance was then carried out on the number of errors per subject (Table I).

TABLE I

ANALYSIS OF VARIANCE OF THE SQUARE ROOT OF THE NUMBER OF ERRORS (N = 21)

Source	*d.f.*	*Mean Square*	*Sig.*
Between Men	20	21·909	P < 0·1%
Between Periods ..	3	41·316	P < 0·1%
Residual	60	0·831	—

It can be seen that the between period differences are significant. The mean root errors for all 21 subjects together with their standard errors are presented for each half-hour in Table II.

TABLE II

MEAN AND S.E. OF THE SQUARE ROOT OF THE NUMBER OF ERRORS IN EACH HALF-HOUR PERIOD (N = 21)

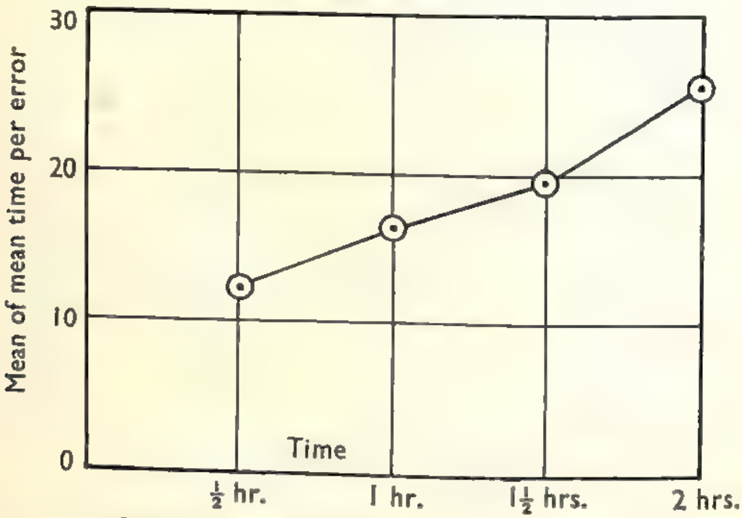
	1st	*2nd*	*3rd*	*4th*
Mean ..	3·21 ± 0·199	4·79 ± 0·199	5·95 ± 0·199	6·32 ± 0·199

A "t" test on this data revealed significant differences ($P < 0{\cdot}1$ per cent.) between the errors in successive half-hours except between the 3rd and 4th, which was not significant.

Duration of Errors.

The mean duration per error is plotted against time in half-hour periods in Figure 2.

FIGURE 2



Mean of mean time per error against time.

Again it was found necessary to perform a similar transformation on the data. Table III gives the analysis of variance data for between men and between periods difference, and Table IV the means and standard errors of the root mean time per error in each half-hour period.

TABLE III

Analysis of Variance of the Square Root of the Mean Duration Per Error ($N = 21$)

Source	*d.f.*	*Mean Square*	*Sig.*
Between Men	20	7·734	$P < 0{\cdot}1\%$
Between Periods	3	9·682	$P < 0{\cdot}1\%$
Residual	60	0·660	—

TABLE IV

Mean and S.E. of the Square Root of the Mean Duration Per Error in Each Half-hour Period ($N = 21$)

	1st	*2nd*	*3rd*	*4th*
Mean	$3{\cdot}12 \pm 0{\cdot}177$	$3{\cdot}90 \pm 0{\cdot}177$	$4{\cdot}21 \pm 0{\cdot}177$	$4{\cdot}75 \pm 0{\cdot}177$

The "t" tests which were applied to the data in Table IV revealed the following significant differences between the half-hour periods.

1st and 2nd $P < 1$ per cent.	2nd and 3rd, not sig.
1st and 3rd $P < 0{\cdot}1$,, ,,	2nd and 4th $P < 1$ per cent.
1st and 4th $P < 0{\cdot}1$,, ,,	3rd and 4th $P < 5$,, ,,

Individual Differences.

Some idea of the great differences between the individuals' abilities to perform the task can be gained from Table V. Here the mean and standard deviation together with the range are given for both number of errors and mean size of error for each half-hour period.

TABLE V

MEANS, S.D. AND RANGE OF NUMBER OF ERRORS AND MEAN DURATION OF ERROR FOR EACH HALF-HOUR PERIOD (N = 21)

	Half-hour											
	1st			*2nd*			*3rd*			*4th*		
	Mean	*S.D.*	*Range*	*Mean*	*S.D.*	*Range*	*Mean*	*S.D.*	*Range*	*Mean*	*S.D.*	*Range*
Number of Errors	15·0	16·2	0–43	28·5	25·2	3–81	42·0	33·4	6–106	46·3	33·3	4–106
Mean duration of Error	12·2	11·0	1·0–47·9	16·8	13·7	4·0–71·1	19·8	15·3	4·1–73·6	25·6	24·9	8·6–123·8

Figure 3 shows the distribution of number of errors for the 21 subjects in each half-hour of the test. The marked shift in the distribution with successive half-hours is apparent.

Both measures of performance, number of errors and mean time per error, reflect these individual differences in ability to perform this task. In Table VI it can be seen that scores on these criteria are significantly correlated, the men having most errors tend to spend longer on each error and *vice versa*.

TABLE VI

PRODUCT MOMENT CORRELATION BETWEEN NUMBER OF ERRORS AND MEAN TIME PER ERROR

	1st half-hour	*2nd half-hour*	*3rd half-hour*	*4th half-hour*
Significance P =	0·662 < 0·1%	0·567 < 1·0%	0·523 < 5·0%	0·374 < 5·0%

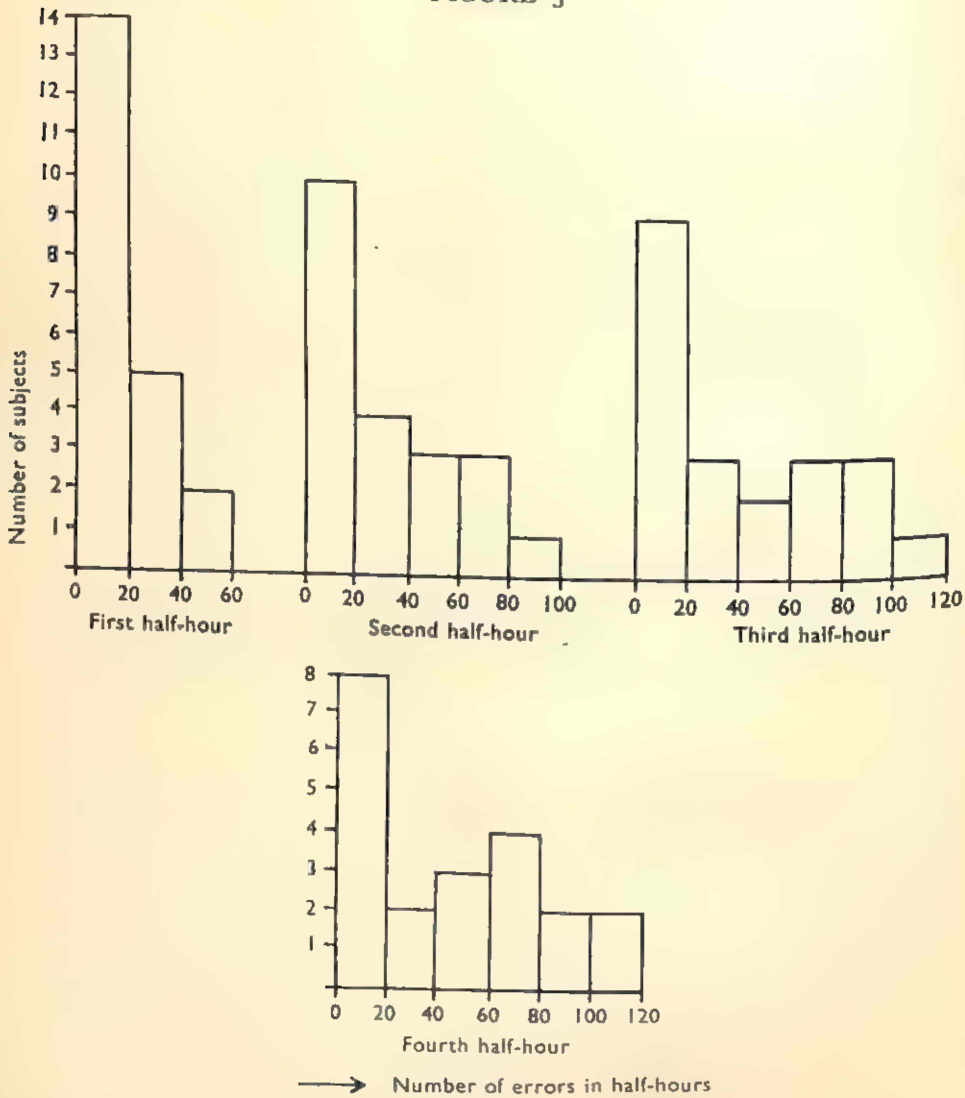
IV

DISCUSSION

It was stated in the Introduction that a large number of errors was to be expected on this task due to the limited variation of sensory input. This hypothesis is supported by the results. Errors rise significantly from half-hour to half-hour, except for the apparently small rise between the 3rd and 4th half-hours (Figure I and Table II).

However, the reason for the levelling out of the error curve is more apparent than real, since Figure 2 and Table IV show that the mean duration of errors also increases throughout the period of the test. Hence for the "poorer" subjects a point is reached where each error occupies so much time that they almost run concurrently and an error ceiling is reached.

FIGURE 3



Frequency distribution of errors.

Almost without exception all the subjects appeared keen and displayed eagerness to know how well they had performed at the end of the run. Accordingly it is concluded that lack of motivation could not account for the results. The only alternative explanation is that fatigue of some kind is responsible.

The hypothesis set up at the beginning of the experiment was that since the task demanded continuous attention to the display, mental blocks or gaps in the perceptual

response would occur, causing tracking to become momentarily uncontrolled. The relatively long periods of time before any errors occurred, and the increasing incidence of errors with time, may be taken as supporting evidence. However, it could be argued that since the subjects were required to crank continuously, the increasing variability in speed of cranking due to muscle fatigue would also account for the results. This argument can be met on three grounds. First, neither the subjects nor the writers in trial runs experienced symptoms of such gross muscular fatigue as would be necessary to account for the deviations in tracking. Secondly, any deviations due to muscle fatigue should have been corrected before the blip moved far enough off target to record an error, providing there was no lapse of attention, since the task was visually controlled. Thirdly, a general subjective belief was evident that the task had been performed better than it really had, and this lends support to the view that failures of attention rather than muscular fatigue accounts for the results.

It is regretted that a more systematic method of recording the subjective estimations of performance was not made at the time of the experiment, since as the target moved off centre until corrected, some knowledge of results was provided which might have been expected to give the subjects a fair estimation of performance. In addition to poor performance estimations, several subjects believed that the experimenters had caused variable movements of the target during the later stages of the two-hour run. It is perhaps relevant to note that similar unreliable estimations of performance and belief that the apparatus had broken down are reported by Davis (1948).

Considerable individual differences were found both for the number of errors and the mean time per error. As would be expected the differences in performance ability were maintained throughout the test so that those who did best in the first half-hour also did best in the last half-hour. In future work it is hoped that individual differences might be related to personality differences such as the inert and overactive classifications suggested by Davis. However, the small number of subjects tested and the lack of valid checking data makes a full analysis of the present experiment along these lines fruitless.

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THE WARMING-UP EFFECT IN RECALL PERFORMANCE

BY

IAN M. L. HUNTER

From the Department of Psychology, Edinburgh University

Two experiments on recall are reported. In the first, 120 adults recall, in successive 3-minute sessions, names which begin with a particular letter of the alphabet, a different letter being used in each session. The second experiment differs from the first in interpolating a 20-minute lecture between the first and second sessions. The major finding is that the performance of one recall task (e.g. recalling names which begin with N) facilitates the performance of a subsequent and similar, but not identical, recall task (e.g. recalling names which begin with K), provided the two tasks occur within a relatively short time of each other. This facilitation has the characteristics of warming-up and suggests that recall may, if only for heuristic purposes, be regarded as a high-level skill. Two minor findings are reported which indicate problems for future investigation.

I

Introduction

When the human organism undertakes the performance of a previously acquired skill, initial execution tends to be irregular, slow, and inexact. But, as the performance is persisted in, it also regains its former efficiency. Doubtless, athletes and musicians have always been familiar with this phenomenon. Psychologists have known it, for some half a century, as the "warming-up" effect. "The best definition of 'warming-up' as an objective act is that part of an increase of efficiency during the first 20 minutes (or some other assigned early portion) of a work period, which is abolished by a moderate rest, say of 60 minutes. Such warming-up should show itself clearly in individuals at or near the limit of practice, and, in others, should compound with the effect of practice to make the rise in efficiency especially rapid in the first 20 minutes of work, or the fall (supposing the function to diminish in efficiency) specially slow in this same period. What time is assigned in the definition of warming-up effect is of little consequence to the investigation so long as *some* time is assigned." (Thorndike, 1914, p. 66.)

Recently, Ammons (1947) has undertaken a detailed quantitative and theoretical analysis of warming-up as it occurs in motor performance on Snoddy's mirror drawing apparatus and the Koerth rotary pursuit apparatus. He suggests that warming-up is due to the regain of a "set" which consists principally of various advantageous postural adjustments and is additional to that produced by the experimenter's (or the subject's own) instructions. It follows that part, at least, of what is called forgetting is attributable to loss, over the "rest" period, of an optimal performance "set." Irion (1948) has argued that warming-up is also relevant to the recall and relearning of verbal material. Furthermore, he has verified his assertion experimentally. Irion (1949) presented two groups of adults with a list of 15 paired associates (adjectives) on a memory drum. Twenty-four hours later, he had both groups relearn the same list, the number of correct anticipations on the first relearning trial providing a score of (aided) recall. Immediately before relearning, however, one group had a single trial of naming colours which were presented on the drum in the same way and at the same speed as were the adjectives. The other group had no such colour-naming trial and, in consequence, recalled significantly fewer adjectives than did the first group. What must have happened is that the colour-naming trial enabled

warming-up to take place, enabled the subjects to regain some sort of performance "set" which had been lost during the interval between learning and relearning. The subjects must have regained some proficiency in those non-specific activities which characterize any instance of memory-drum work, e.g. looking at the aperture and recalling items at rhythmic intervals.

It cannot be doubted that Ammons and Irion have established their point, at least, that is, as regards one type of recall performance. But, so far as the writer knows, no attempt has been made to test the applicability of their arguments to another type of recall—one which is more common, more complex, and more characteristically human. The difference between these two types of performance is mainly this: in the one, recall activity and original acquisition activity are much the same whereas, in the other, the two activities are appreciably different. In measuring the recall of a motor performance or of a list of adjectives learned by the anticipation method, the subject is not asked to do anything which he did not do during acquisition. He simply continues, after an interruption, his original learning activity. Even the activity of recalling a poem may not be so very different from that of learning it, despite the fact that the one involves reading or listening while the other involves speaking or writing. But when the subject is asked to recall, say, the names of several shops all of which sell ties, his recall activity clearly differs from that involved in original learning. This is the type of recall which typically occurs during thinking. In it, past experience is broken down into constituent parts. The request to recall does not become, in the words of Bartlett (1932), "merely a cue setting up a series of reactions all carried out in a fixed temporal order, but a stimulus which enables us to go direct to that portion of the organized setting of past experiences which is most relevant to the needs of the moment" (p.206). It is this type of recall performance which Bartlett has characterized by saying that, in it, the subject must "turn round upon his own schemata." Although there seems to be no sharp dichotomy between these two classes of recall activity, it is convenient, for purposes of discussion, to distinguish them by name. The former might be referred to as "rote recall" and the latter as "productive recall."

That productive recall is susceptible to warming-up is suggested, but not demonstrated, by two considerations. The first concerns simply an extrapolation of Irion's findings regarding rote recall. The second concerns a not unreasonable assumption which is implicit in this paper's treatment of recall. Here, recall is regarded, not as a mere technique for measuring "retention," but as a skilled activity of literally vital importance in its own right. It is treated as Bartlett (1950) has recommended that thinking should be treated, i.e. as a high-level skill. This treatment makes feasible a comparison between the little-understood activities of recall and the better-understood activities involved in motor skills. It implies, further, that many phenomena are common to both classes of skill. And, one of the most prominent of these common phenomena would, presumably, be the warming-up effect.

II

Experiment One

The purpose of this experiment is to establish the existence of warming-up in the performance of a relatively complex recall task.

Subjects and Procedure

One hundred-and-twenty first-year psychology students (53 men and 67 women) acted as subjects as part of a course in experimental psychology at Edinburgh University. The subjects attended in six groups of 20 students each. The procedure for group I was as

follows. Each subject was given three sheets of paper and asked to write his name on each sheet. The following instructions were then read out to the group. "In this experiment, you will be asked to write down three lists of names, making each list as long as you possibly can. You will be given three minutes in which to write down the first list on the first sheet of paper and, when you have done this, your list will be collected and you will be given another three minutes to produce the second list. When your second list has been collected, you will have three minutes in which to produce the third and last list. Before beginning each list, I will give you a single letter of the alphabet and your task will be to write down as many as you can of the surnames and christian names which begin with that particular letter. For example, if I gave you the letter B, your list might run as follows:—Brown, Bernard, Barbara, Bertrand, Bob, and so on. Put down all the surnames you can think of beginning with a given letter and also all the christian names, both boys' and girls', even though some of them are dimunitives or derivatives of the others. Thus, Betty, Betsy, Bess, and Bessie would, each one of them, count as a different name. You needn't put the names down in any special order. Just write them as they occur to you. But do try to make your list as long as possible. For each of the three lists, you will be given a different letter. Now are you quite clear about what you have to do? (Pause for questions.) In the first list, then, you must write down only those surnames and christian names which begin with the letter I shall now give you and you must try to produce as many of these names as possible. Are you ready? The first letter is K." As the experimenter pronounced the letter K, he wrote it on the blackboard and started the stop-watch. Three minutes later, the subjects were told to stop and the experimenter walked round the group collecting each list. This collection took about 45 seconds. The group was then told:—"Now I am going to give you the second letter. Remember to write down all the surnames and christian names you can think of beginning with this new letter. Are you ready? The letter this time is J." Again the letter was written on the blackboard, three minutes were given for recall, and the lists collected. This done, the group was instructed:—"Now I am going to give you the third and last letter. As before, write down all the surnames and christian names you can think of beginning with this letter. Ready? The third letter is N." When the third list was collected, the experiment was concluded.

The procedure for the remaining groups of subjects was identical with that above, except that the three stimulus-letters were given in each of the other possible orders. On completion of the experiment, each group was asked not to discuss it with other members of the first year class. And, since some groups performed the experiment simultaneously and all groups performed it within two days, it seems unlikely that many students learned anything about the experiment before taking it.

Results

Each individual recall list was scored on the basis of one mark for each phonetically different name. Thus, "Jonson" and "Jonstone" scored one mark each, but "Jonson" and "Johnson" scored only one mark in all. To discover whether the first recall session influenced the second, it is not, of course, sufficient to contrast the second list of any one subject with his first. The reason for this is that it cannot be assumed that, even under comparable conditions, a subject would recall as many K-names as, say, J-names. The score on the second session must be contrasted with that on the first session in which the same stimulus-letter is employed. The design of the experiment enables this to be done. Thus K was presented first to groups I and IV, second to groups II and VI, and third to groups III and V. So, in like manner, with the other two stimulus-letters. Any one stimulus-letter was, in short, presented first, second, and third, respectively, to three different groups of 40 subjects each.

Table I shows the mean score for each such group. Also shown is the standard error of each mean. Shown, too, are the grand mean scores made on each recall session (position) irrespective of the stimulus-letter used and, conversely, the grand mean scores made using each stimulus-letter in all three positions. These grand means are derived from the original data and not from the means entered in the body of the table.

Since the 120 subjects are symmetrically distributed throughout the 9 cells of Table I, the data may be subjected to an analysis of variance. The outcome of this analysis is shown in Table II. (In this table, as in Table III, only three significance levels are employed, i.e. 1 per cent., 5 per cent., and not significant.) The most

important finding is that the variance attributable to the positions of presentation is significant at the 1 per cent. level. Table I shows that this variance takes the form of an increased score on each successive recall session. In other words, there exists a significant practice effect which verifies the original hypothesis that recall may facilitate later recall through warming-up. The analysis also reveals a very large variance due to the stimulus-letters themselves which is, of course, highly significant. Indeed, the differences between the mean scores made on the three letters is so large (*see* Table I) that they might almost be pronounced significant on mere inspection.

TABLE I

EXPERIMENTAL RESULTS

Stimulus-letter	*Position of Presentation*						*Grand Mean for Letters*
	First Mean	*S.E.*	*Second Mean*	*S.E.*	*Third Mean*	*S.E.*	
K	11·5	0·4557	14·3	0·5578	13·4	0·6929	13·04
N	13·4	0·6810	14·0	0·5606	16·0	0·5891	14·37
J	22·3	0·8177	21·2	0·8696	24·0	0·9563	22·49
Grand mean for positions	15·70		16·46		17·74		

TABLE II

ANALYSIS OF VARIANCE TABLE

Source	*Sum of Squares*	*d.f.*	*Variance Estimate*	*F-ratios*	*Level of Signif.*
Between letters ..	6,280.24	2	3,140.12	158.2	1%
Between positions ..	255·11	2	127·56	6·43	1%
Interaction	155·69	4	38·92	1·96	not signif.
Within sets	6,963·87	351	19·84	—	—

TABLE III

SIGNIFICANCE OF DIFFERENCES BETWEEN GROUP SCORES

Groups	*K1–K2*	*K2–K3*	*K1–K3*	*N1–N2*	*N2–N3*	*N1–N3*
t-value	3·89	1·01	2·29	0·68	2·46	2·90
Level of signif.	1%	not signif.	5%	not signif.	5%	1%
Groups	*J1–J2*	*J2–J3*	*J1–J3*	*K1–N1*	*K1–J1*	*N1–J1*
t-value	0·92	2·17	1·35	2·32	11·53	8·37
Level of signif. ..	not signif.	5%	not signif.	5%	1%	1%

Having established, in over-all fashion, that both the stimulus-letter and its presentation position influence the amount of recall, a number of t-tests can be made to discover the precise sources of these effects. The result of these t-tests are summarized in Table III. Each entry in the first row ("groups") gives the two groups the difference between the mean scores of which is being tested for significance.

For example, the entry "N1–N2" means that the difference is being considered between the mean score of those 40 subjects who were presented with N as the first stimulus-letter and the mean score of those 40 subjects who were presented with N as the second stimulus-letter. The second row gives the t-value of the difference between the means. The third row gives the level at which this difference can be accepted as significant.

Table III provides three new items of information. (1) With regard to position effects, Table I exhibits two exceptions to the general trend of improved recall on successive sessions. These exceptions are the drop in mean score between groups K2 and K3 and, again, between groups J1 and J2. Table III shows that these two inversions are not significant. In fact, the only significant position effects which do occur all represent a progressive improvement in recall performance. (2) With regard to the effect of the stimulus-letters, Table III shows that the average number of J-names is significantly greater than the average number of N-names which is, in turn, significantly greater than the average number of K-names. These differences presumably reflect the relative frequencies with which these three classes of name occur in the everyday use of language. (3) Returning to position effects, Table III reveals that improved recall manifests itself more clearly with some stimulus-letters than with others. The indication is that the recall of K-names is most susceptible to improvement, that the recall of N-names is perhaps somewhat less susceptible, and the recall of J-names is the least susceptible of the three. If this is indeed so, it ties in very neatly with the last-mentioned finding and leads to the conclusion that susceptibility to improvement is inversely related to the amount recalled, i.e. to the ease of recall. Apparently, the more difficult the recall the more likely it is to be facilitated by prior warming-up. Unfortunately, the present data yields no further information about this conclusion. But the conclusion, if valid, seems important and will be taken up again in Discussion.

It was found above that the recall of K-names and N-names is facilitated more than is the recall of J-names. The next question is whether the recall of those classes of name might not produce different amounts of facilitation in subsequent recalls. Consider groups III and IV. Both groups recall N-names in the second session. But, in the first session, group III recalls J-names and group IV recalls K-names. Now, if the recall of J-names produces more warming-up, this would lead, in the second session, to a superior performance from group III. Accordingly, t-tests were made to discover the significance of the difference between the mean scores, in the second session, of groups III and IV, of groups I and V, and of groups II and VI. Each of the three differences involved proved to be both small and insignificant. It must, therefore, be concluded that later recall is facilitated to an equal degree by recalling K-, N-, and J-names alike.

Summary of Results

This experiment demonstrates (1) that performance of one recall task facilitates performance of a subsequent and similar recall task at least when the two tasks are separated by about 1 minute, and (2) that facilitation seems to be more evident the more difficult the second task but is, within the limits here investigated, independent of the difficulty of the first task.

III
Experiment Two

If the facilitation found in the previous experiment is to be regarded as a warming-up effect and if Thorndike is correct in characterising warming-up as being "abolished by a moderate rest," then it follows that amount of facilitation ought to bear some

inverse relation to the length of time interpolated between the first and second recall sessions. The purpose of this experiment is to test this expectation by, in effect, repeating the previous experiment with an interval of about 20 minutes between successive recall sessions. The prediction is that the second recall performance will show less improvement than it did in the previous experiment.

Subjects

The Subjects were 88 women students of Moray House Training College for Teachers.

Procedure and Results

The students, in two groups of 44 each, took the experiment during one of their usual class periods. Each of the groups was split into two sub-groups, one sub-group sitting at one side of the room and the other at the other side. On being introduced to the class by the regular lecturer, the experimenter issued each subject with a sheet of paper on which she was required to write her name. With group I, instructions were given as in the previous experiment except that mention was made of only one recall list. Both sub-groups were then given the stimulus-letter N and three minutes in which to recall the appropriate names. The experimenter then collected the lists, thanked the class for its co-operation, and left the room without any hint that he was to return later. The class then had a 20-minute lecture on developmental psychology. At the end of this lecture, the experimenter returned, issued each subject with a fresh sheet of paper, gave the instructions once more, and gave the second stimulus-letter. In this instance, one sub-group was given N and the other was given K. It was explained to the former sub-group that the list of N-names was to be as complete as possible, including both names previously recalled and any new ones which occurred. When the three-minute recall session was over and the lists collected, the experiment was complete. An identical procedure was adopted with group II except that the stimulus-letters were given in reverse order and the 20-minute interval was filled by a lecture from the experimenter on developmental psychology. But, although the experimenter did not leave the room, he did attempt to suggest, after the first recall session, that the experiment was over. To avoid communication between the two groups, it was arranged that they take the experiment in consecutive class periods.

The order in which the stimulus-letters were presented to the four sub-groups is shown in Table IV along with the mean and standard error of the scores made by each sub-group during each session.

TABLE IV

Design and Results of Experiment Two

Group	No. of Subjects	First Session			Second Session		
		Letter	Mean	S.E.	Letter	Mean	S.E.
Ia	22	N	13·8	0·8686	K	13·4	0·8139
Ib	22	N	13·6	0·8882	N	21·4	1·386
IIa	22	K	13·1	0·5478	N	16·0	1·021
IIb	22	K	13·1	0·6001	K	19·7	0·8379

To discover whether the warming-up effect persists over the lecture period, the mean K-recall score (on the second session) of sub-group Ia is compared with the mean K-recall scores made by sub-groups IIa and IIb (on the first session). Likewise, the mean N-recall score of sub-group IIa is compared with the mean N-recall scores made by sub-groups Ia and Ib. None of the t-values of the differences involved are found to approach anywhere near significance. The failure to find any facilitation in the recall of K-names is particularly relevant since it contrasts with the outcome of the

previous experiment in which the mean K-recall score on the second session exceeds the mean K-recall score on the first session by an amount which is significant at the 1 per cent. level. Now, since the major difference between the two experiments lies in the length and nature of the activity interpolated between successive recalls, it must be concluded that the warming-up effect is abolished by 20 minutes spent in listening to a lecture. The hypothesis with which this experiment began is confirmed.

Turning now to sub-groups Ib and IIb, it is seen that, in each of these, the mean recall score on the second session is greater than that on the first. The difference between the two correlated means furnished by sub-group 1b is significant at the 1 per cent. level ($t = 7 \cdot 38$) as is also the difference between the two correlated means of sub-group IIb ($t = 10 \cdot 37$). So, while the recall of, say, N-names on the second session is not facilitated by the prior recall of K-names, it is definitely facilitated by the prior recall of N-names. It is also worth recording that, as Brown (1923) found in a very similar experiment, not all the names recalled in the first session are also recalled in the second. Thus, for sub-group Ib, of the mean of 13·6 names recalled in the first session, a mean of 10·8 are also recalled in the second session, while a mean of 2·8 names drop out and a mean of 10·6 new names make their appearance.

Summary of Results

This second experiment demonstrates that (1) the interpolation of a 20-minute lecture between the performance of the first and second recall task abolishes the facilitation shown in the previous experiment, and (2) this 20 minutes of interpolated activity does not abolish facilitation when the two recall tasks are identical and not merely similar.

IV

Discussion

It may seem strange to some psychologists that the present investigation should omit to control such an obviously important set of circumstances as the conditions under which original learning took place. This control might easily be achieved by, for example, reading out to subjects the names of a large number of objects and then asking different groups of subjects to recall, first, these names of objects which belonged to one class, second, these names of objects belonging to another class, and so on. An experiment of this sort might be worth doing, especially if it employed the names of objects classifiable in more than one way. But it is not perhaps optimally suitable for the purpose of this investigation which is, very simply, to demonstrate as clearly as possible that productive recall performance does actually manifest warming-up. Apart from the labour entailed in preparing and presenting lists of names, the experiment has two possible disadvantages. First, it involves recently learned material which would be particularly susceptible to retro-active interference during the recall sessions. This would introduce a factor which not only complicates the situation but actually militates against the demonstration of inter-session facilitation by tending to depress recall scores made in the later sessions. Second, it reduces the complexity of the recall task by restricting the time and place of acquisition, by circumscribing, to use Duncker's (1945, Ch. 6) metaphor, the "regions of search," which the subject must "peruse." In contrast, the investigation reported involves the recall of material learned years before in the course of the subject's everyday commerce with his environment. This lessens both the artificiality of the task and the possibility of forgetting during the recall sessions. It also sets a more complex task by multiplying the "regions of search" to be "perused," by requiring the reproduction of what was originally perceived not in one situation but in many. Because

of this greater complexity, the task is, presumably, likely to benefit not only more from practice but less immediately. In other words, it is likely that the optimally effective recall techniques (or "set") would not all be brought into play at the very beginning of performance.

The two experiments reported seem to justify the following major conclusion. The performance of one recall task facilitates the performance of a subsequent and similar recall task, provided the two tasks take place within a relatively short time of each other. This conclusion could be left simply as a statement of fact. But it is usual in science to attempt interpretation in one or both of two ways. One of these seeks to correlate one fact with a number of others and show that one phenomenon is an instance of a more general class of phenomena. Here, such a correlation may be made with relative ease. The demonstrated facilitation takes place between tasks which have, in common, only characteristics of a rather general sort. Thus, the improvement may be described as a warming-up of the performance of, if not productive recall in general, then recall-of-names-beginning-with-a-particular-letter. The facilitation may, then, be classified along with warming-up of skilled motor performances. And, to this extent, the original assertion that recall and motor performance are activities of a kind is reinforced. The second, and more ambitious, mode of interpretation seeks to go beyond the level of observation into the realm of theory and to explain the observation by deriving it from the theory. However, the experimental findings seem insufficiently extensive to support any interpretation of this sort.

One point on which more information is required concerns the relation between susceptibility to warming-up and the difficulty of the recall task. Checking the validity of this indication would seem an important preliminary to the construction of any theory about recall performance. Recall performance differs from skilled motor performance in the important respect that, as the performance progresses, it becomes increasingly more difficult. The performance of, say, tracking is repetitive in a way which recall is not since the population of items which can be recalled is not infinite and, so, the more items a subject recalls, the fewer new ones there are left to recall. This diminishing efficiency of recall has been demonstrated by Bousfield, Sedgewick, and Cohen (1952). They presented different groups of subjects with a list of 60 nouns, varying the number of presentations given to each group. In recalling these names, it was found that speed of recall increased according to the number of original presentations, i.e. according to the material's familiarity. But it was also found that, in all groups, items were recalled most rapidly at first and then appeared at longer and longer intervals, i.e. the task became progressively more difficult. Suppose, then, that the present experiments had employed recall sessions, not of 3-minute duration, but of, say, 1-minute or 5-minute. The shorter sessions would have decreased the over-all difficulty of the tasks and the longer sessions would have made them more difficult. Hence, if there exists a direct relation between difficulty and warming-up, it would be predicted that lengthening the sessions would produce a greater amount of inter-task facilitation while shortening the sessions would diminish such facilitation. This prediction runs contrary to what would be expected if warming-up were held, as it usually is in motor performance (Ammons, 1947, p. 272), to increase according to a negatively accelerated growth curve. It may be, then, that recall and motor performance are alike in only a limited sense. Consequently, classifying them together serves only the heuristic function of indicating in which respects they are similar and in which different. Perhaps, too, the results of the present experiments will have to be interpreted in terms of a warming-up which operates in conjunction with some other process or processes peculiar to recall.

Another point on which information is inadequate concerns the last finding of Experiment Two that the performance of a recall task is strongly facilitated by an earlier performance of the same recall task. This finding involves all the complexities of interpretation which have attached to the results of any investigation hitherto conducted into the effect of recall upon the subsequent recall of the same material. These investigations, ranging from Ballard (1913) to Ammons and Irion (1954), have uniformly found that the second recall is facilitated by the first. This facilitation could represent, in whole or in part, a warming-up effect. Such a warming-up would, presumably, be stronger than that discussed in the body of this paper since it involves two recall tasks which are identical and not merely similar. Other interpretations are, however, possible. Thus, Brown (1923) has explained the facilitation in terms of a specific practice effect. He points out that recall is affected by chance factors which vary from moment to moment and that, due to adverse factors, the number of items recalled at any one time is less than the total which could be recalled. So, by mere chance, a second recall session would yield a list of names somewhat different from that produced on the first session. Also, it is possible, in the second session, to recall something of the performance achieved in the first session. "The second list will therefore be more extensive (under suitable conditions) than the first list because it contains those items which are fairly sure of recall in any list, plus a certain number of items subject to chance which failed to appear in the first but do by chance appear in the second, plus in addition items which by chance appeared in the first list and might not have occurred in the second, but which do occur in the second because of being reviewed or recited in the first recall." (Brown, 1923, p. 378). This specific practice effect would be especially important if the first recall induced the subject to undertake, during the "rest" interval, further recalls or imaginal reviews of the material; or, again, if it selectively sensitized the subject to recognize these fragments of the material which chanced to occur in his environment. (Instances of such selective sensitization seem to be apparent in the protocols of Myers and Myers, 1916.) The existence of these different mechanisms points up the complexity of the situation in which the second recall involves the same material as the first and suggests why, as regards the results at present under consideration, the available data is insufficient to show which one, or which combination, of these mechanisms is operative.

In view of the above discussion, the writer feels justified in not committing himself to any theoretical interpretation. Investigations are at present under way in Edinburgh University which may do something to remove the above-mentioned inadequacies in the present-day knowledge of recall. Meantime, the writer must confine himself to making, in this paper, an almost purely factual contribution.

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THE MEASUREMENT OF DISCRIMINABILITY

BY

E. R. F. W. CROSSMAN

*From the Psychological Laboratory, Cambridge**

The need for a quantitative measure of "Discriminability" is pointed out. A formula is required which will enable one, knowing the physical measurements of a set of sensory signals, to predict the time taken by a human observer to recognize any one. Existing formulæ relate to the two-choice threshold task and to very "easy" multi-choice ones. A more general treatment is attempted in order to cover the region between these two special cases.

For the limited case of two-choice one-dimensional signals, three functions fulfilling the requirements have been derived from theoretical considerations and tested against observed times for visual and proprioceptive sorting tasks of various levels of "difficulty." The function:

$$C\ (1,\ 2) = \frac{K}{|\log. x_1 - \log. x_2|} \quad \text{where} \quad \begin{array}{l} x_1, x_2 = \text{physical sizes of signals, } 1,\ 2 \\ K = \text{an arbitrary constant} \end{array}$$

was chosen as fitting the data most adequately, and has been named the Confusion-function. A method of extending its use to multi-choice and multi-dimensional signal-sets is outlined and experimental results concerning the former are presented.

The relation between this function and the Weber-Fechner law is discussed; applications in the study of Information, span of apprehension, and learning of sequences are outlined.

I

Introduction

The study of discrimination is at least as old as experimental psychology itself, and the literature voluminous, yet no writer has put forward a quantitative theory which can predict the degree of discriminability of sensory signals from a knowledge of their physical characteristics. The nearest approach to a solution of this basic problem in psycho-physics is still the oldest, the classical Weber-Fechner law, which even if it be accepted as theoretically valid is of limited application only. There are many kinds of discrimination—easy or difficult; between two or more possibilities; with one or more than one dimension of variation of the sensory signals—but this law covers only the case of signals at threshold occurring in a two-choice situation and varying in one respect only. The present paper puts forward a fairly simple theoretical formula which will, in principle at least, cover a much wider field. The new formulation was reached by a quite different route from that which led to the Weber-Fechner law, but agrees with it, and indeed includes it as a special case.

The experimental work to be described does not go far beyond the classical two-choice one-dimensional discrimination task. Our concern is with response-time for signals well above the threshold; in other words, with "easy" tasks done at high speed. But the technique devised to study this problem can easily be adapted to more complex tasks; a preliminary result on multi-choice discrimination is given and the way in which the theory extends to such cases is indicated. In the course of earlier work (Crossman, 1953) on the transmission of information by the human subject, it was observed that the physical characteristics of the information-bearing signals, in this case playing-cards, affected the time taken to respond to them. In several kinds of task, an approximately linear increase of response-time with information content

* Now at the Department of Engineering Production, University of Birmingham.

had previously been reported by Hick, Hyman and others (Hick, 1952; Hyman, 1953). The word "information" is, of course, used in its technical sense and its amount is measured simply by the negative logarithm of signal probability which is the independent variable in these studies. Yet in our experiments different response-times could be obtained for constant information-content by altering the "difficulty" of the signals. Clearly this second variable which was tentatively labelled "discriminability" should be held constant in studies of information-rate, otherwise one's results might be due to its variation, and not at all to difference of information. But no measure of discriminability seemed to have been proposed by means of which one could maintain its constancy. Also, as will be shown later, information-content and discriminability of signals were seen to be in principle interdependent and not wholly distinct. The present study was undertaken in an attempt to provide a discriminability measure and so to put information studies on a firmer footing.

A study of the literature on discrimination revealed a very large volume of work. Space does not permit of an adequate summary here, and in any case a surprisingly small amount of work has been found to bear at all directly on our present problem. Attention has in the past been concentrated on threshold phenomena (*see*, for example, Woodworth, 1950, Chapter XVII) and on the process of learning discriminations and setting up frames of reference (Helson, 1948). The discrimination of signals well above the threshold has been neglected; work in the applied field directed toward improving visual and other displays (for example, Bartlett & Mackworth, 1950) comes into this category, but the results do not lend themselves to the kind of quantitative treatment which would lead to a general theory. Two studies, which both employ the classical method of reaction-times, were alone found to meet our requirements, and they form the starting-point of our present work.

TABLE I

Results of Henmon (Henmon, 1906) for Two-choice Discrimination Tasks.

Lines (mm)	Time (msec)	P.E.	N	Errors	Time	P.E.	N	Errors	C
(1) Discrimination between lines of Constant ratio.									
	Subject H				*Subject S*				
20/24	303·85	1·12	320	6	323·00	1·34	320	16	3·82
15/18	307·73	1·10	,,	4	323·50	1·41	,,	26	,,
10/12	309·71	1·17	,,	3	328·50	1·37	,,	16	,,
5/6	314·98	1·11	,,	9	337·40	1·36	,,	29	,,
(2) Discrimination between lines of Different ratios.									
10/13	295·89	0·87	400	5	328·78	1·23	400	14	2·64
10/12·5	298·41	0·81	,,	7	328·57	1·34	,,	22	3·11
10/12	304·70	0·84	,,	12	331·98	1·30	,,	14	3·82
10/11·5	312·55	0·93	,,	14	338·97	1·35	,,	19	4·83
10/11	323·96	0·97	,,	15	346.38	1·37	,,	42	7·35
10/10·5	344·71	0·98	,,	13	358·28	1·48	,,	37	14·39
(3) Discrimination between Tones above and below 500 c/s.									
Tones (c/s)	*Subject H*				*Subject W*				
516/484	290·22	1·28	320	4	344·27	1·93	320	4	10·8
512/488	298·73	1·32	,,	4	354·76	2·12	,,	7	14·4
508/492	311·06	1·34	,,	6	395·67	2·38	,,	10	21·7
504/496	334·06	1·38	,,	10	470·20	3·42	,,	24	43·7

Henmon (1906) intended to develop the use of reponse-times as a measure of the difference of sensations. His experimental method was as follows. The subject sat facing a screen with his left and right hands resting on two keys. Paired stimuli (e.g. lines 10 and 13 mm. long) were exposed on the screen and he had to release the appropriate key (e.g. on the side of the longer line) as quickly as possible. The time between exposure and release was recorded on a Hipp chronoscope. Three subjects

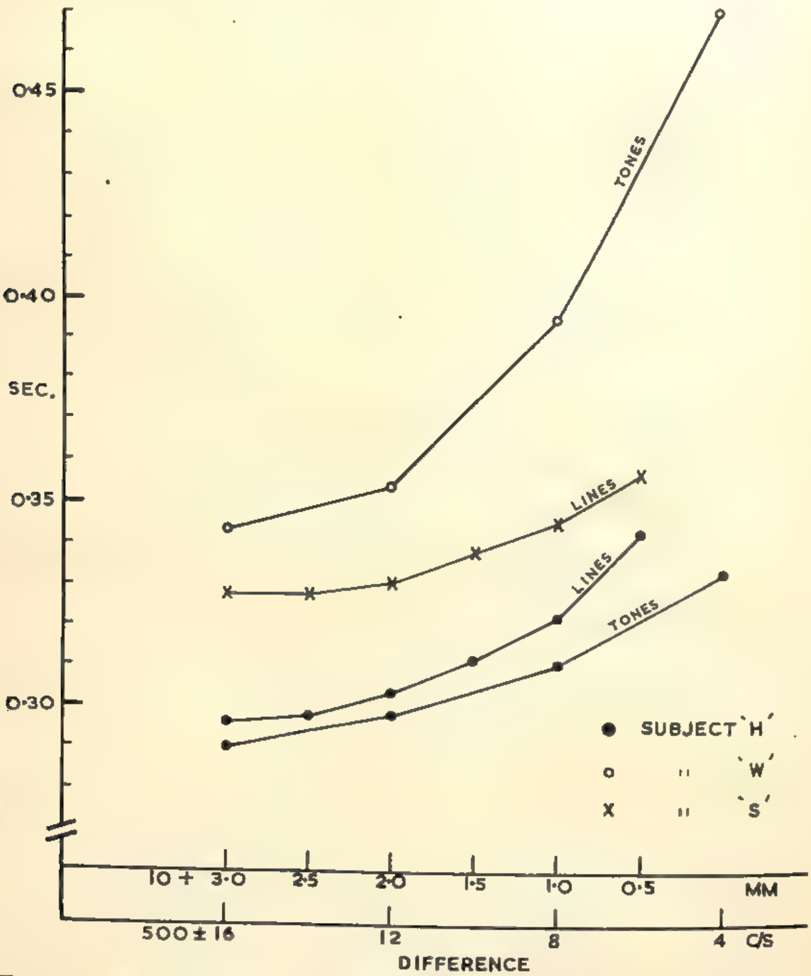

FIG. 1. Results of Henmon (1906). Discrimination time for paired signals of various degrees of difference.

served and many thousands of responses were recorded. Stimuli of several types and of various degrees of difference were given using lines, colour-mixtures, and tones produced by tuning-forks; in all cases the response-time increased as the stimulus-pairs were made more alike (Table I, Fig. 1). Henmon unfortunately did not attempt to draw theoretical conclusions from his figures, remarking in a footnote that the difficulties involved in a mathematical treatment are too great. He does, however, mention that "it should be possible to apply to these results Fechner's *Unterschieds-formel* and correlate the increase in magnitude of stimuli inversely with the increase in time of perception and thus test the psycho-physical law."

Lemmon (1927, reported in Woodworth, 1950, p. 335) in a study concerned with the correlation between reaction-time and mental characteristics such as intelligence, used a similar experimental method to that described above, except that the stimulus-pairs were groups of lights differing by one. These appeared on panels side by side, the subject being asked to react to the larger group. The pairs he used were 0/1, 1/2, 2/3, 3/4 and 4/5, and times were found to increase in that order; but one cannot draw conclusions from the actual amount of the increase, since Lemmon deliberately chose arrangements giving "equal increments of discrimination-time from grade to grade."

Since the time of the authors mentioned above, a marked change of emphasis has taken place in the study of psycho-physics and of reaction-times. Instead of a *stimulus* causing a *reaction* when the *threshold* is exceeded, we now think rather in terms of a *signal* which may be obscured by *noise*, providing the *information* needed to *select* a response. Nor is the change merely one of words. In the first place a *signal*, unlike a *stimulus*, necessarily implies alternatives, and it is the set of possible signals (the context) not the individual one, on which our attention is focussed. Secondly, signals must be generated by some process which from the point of view of the subject is random and whose statistical properties determine the information-content of the signals. Thirdly, any errors made by the subject imply loss of information in a calculable amount. Fourthly, the responses are not considered important apart from the information in them and can be of any complexity so long as the set of them is defined and they can be adequately timed. Fifthly, the time of occurrence of a signal is itself seen to be a signal carrying information in the time-dimension; this information must either be abolished by giving a signal at a known time or be dealt with separately. Sixthly, a set of signals can be translated or *coded* into many physical forms without change of information-content.

It is this last point which concerns us here; information reaching the human senses must be encoded on continuous variables, the pitch and timing of sounds, the spatial distribution of light and shade, and so on. The way in which the encoding is done affects the time taken to respond to the signal and in particular the more alike the signals in a set the more time is needed to separate them. We seek a formula to express this dependence of response-time on similarity when information-content is constant.

Several lines of argument leading to algebraic formulæ have been explored. Of these, three fitted the data of Henmon and of the experiments described below reasonably well, although one was considered to be better in the end. The experimental method used in this study was developed by adapting to the method of card-sorting used in the writer's earlier experiments, the type of display used by Lemmon (1927); it also owes something to the classical tachistoscopic experiments for studying span of apprehension.

II

Experimental Method (Card Sorting)

The signals to be discriminated were patterns marked on the faces of playing-card blanks and discrimination time was measured by the time subjects took to sort the pack into the correct classes. Each trial proceeded as follows: the subject was handed face downwards a well-shuffled pack and asked to sort it into two (or more) heaps according to pattern. He was instructed to turn up cards one by one with the right hand; to leave any mistakes he might make uncorrected; and to work as fast as he could without making more than one or two errors per pack. His performance was timed on a stop-watch to the nearest 1/5 sec.; errors were noted, any trial in which more than 10 per cent. errors occurred was discarded from the experimental results. In this task the subject normally makes a few mistakes on even the simplest task if he is performing at top speed. These were recorded, but no allowance was made for them in calculating performance. In theory,

the correct way would be to calculate the information-loss due to errors and adjust the observed time appropriately upwards. But in practice a trial in which a large number of errors are made is usually slower, not faster; it seems therefore that errors have more effect on performance than the simple application of Information-theory would lead one to expect, and in the opposite direction. The most satisfactory way of handling them seemed to be to attempt experimentally to keep them constant on the average for all subjects, and then leave them out of the calculation. Movement-time was obtained from a task in which the subject dealt a pack into two piles without sorting.

The patterns on the cards (the "signals") were of two types; for Type (i) a random group of black spots and another of circles, each spot or circle 3 mm. in diameter, were placed side by side on the card. The spots and circles were arranged differently on each card of a pack but the total number was constant; the spots and circles differed in number by a fixed amount. The subject was instructed to sort into the two classes, "more spots" and "more circles." The arrangement was intended to reproduce Lemmon's display, but it was found that the subjects ignored one or other of the groups and sorted by number of one group alone. Use was later made of this observation to obtain a simpler and more flexible task. The Type (ii) cards had each a single group of spots, placed as before at random. A pack of 20 cards was made up for each number of spots from 1 to 18, and also a pack of blanks. Any given condition then required two such packs shuffled together. The subject had two sample cards placed before him, and was instructed to sort the pack of 40 cards into the two corresponding classes. Subjects soon learned to sort without reference to the samples, setting up (apparently) internal "templates" which rendered the external ones unnecessary.

III

Hypotheses and Tests

(a) *Information Theory of Measurement.*

Since our own problem arose out of an Information Theory study, the first hypothesis to be tested was derived from the theory of the information content of measurement as given by Wiener (1948, p. 75). Very briefly the argument ran thus. In order to decide which of two quantities x_1 or x_2 (length of lines in Henmon's experiment, for example) is the larger, the subject must in some sense make a measurement of one, say x_2, using the other, x_1, as a standard. If the answer is greater than 1, x_2 is the larger and vice-versa. The measurement need only be precise enough to distinguish them, and a sufficient condition will be that the probable error is less than the actual difference $(x_1 - x_2)$. He thus makes a measurement giving "a posteriori" uncertainty in a range $(x_1 - x_2)$, the original range being simply x_1. According to Wiener, the corresponding information should not be far from $\log_2 \left(\frac{x_1 - x_2}{x_1}\right)$ Bits, and if it is extracted at a constant rate, the corresponding time t_d should be given by

$$t_d = - A \log_2 \left(\frac{x_1 - x_2}{x_1}\right) \quad \ldots \quad (1)$$

The values of this function were calculated for the various conditions of Henmon's and Lemmon's experiments. The resulting curves were found to approximate quite closely to straight lines. Since the above expression is a measure of information, the constant A is the reciprocal of an information rate (in bits per second; when $x_1 = 2x_2$, the discrimination involves 1 bit). In addition to the information in measurement, there is selective information required, for the subject must (*a*) choose one of the presented quantities as standard, and (*b*) choose one of two responses, given the answer from the measurement. The selective information would presumably be dealt with at about 5 bits/sec. and one is led to ask whether the measurement information is also. Therefore, the constant $1/A$ was estimated from the slope of the lines, and found to

be about 5 for Lemmon's data on number of lights and about 55 for Henmon's data on lines.

The prediction was then made from Equation (1) that t_d should depend only on the *ratio* of the two quantities, and not on their absolute size, for if x_1, x_2 are both multiplied by, say, k, the new time

$$t'_d = -A \log_2 \left(\frac{kx_1 - kx_2}{kx_1}\right) = -A \log_2 \left(\frac{x_1 - x_2}{x_1}\right) = t_d$$

and the time remains unaltered.

An experiment of Henmon's confirms this (*see* Table I) for discrimination between lines of ratio 1·2 : 1. An experiment done with cards of type (i) also confirmed it; a later experiment with type (ii) cards is reported here as being more satisfactory.

Experiment I. Signals of Constant Ratio and Varying Magnitude.

The experimental method was as described above; cards of type (ii) were used. The pairs were 1, 2; 2, 4; 3, 6; 4, 8; 5, 10. Four young adult subjects (naval ratings) were given 15 trials of mixed practice and then 3 trials each of the 5 experimental tasks. The residual effect of practice was partially balanced out by varying the order of presentation. Results are given in Table II and Figure 2. The absolute magnitude of the signal has, apparently, some effect, smaller signals taking longer, but the rise is not statistically significant. Similar results have been obtained for other ratios, and for the weight-judging experiment described later. In no case has a statistically significant regression on signal magnitude been obtained.

TABLE II

TIMES FOR SORTING BY NUMBER OF SPOTS. CONSTANT RATIO

	1/2	2/4	3/6	4/8	5/10	*Prearranged*
Subject *K*	36·6 (3) 36·0 (1) 34·6	34·8 35·2 37·0	36·0 34·2 33·8	34·4 33·0 35·4	32·2 35·4 34·0	21·4 22·7
,, *S*	37·6 (2) 36·2 (2) 37·0 (2)	34·8 35·5 35·0 (1)	35·8 (2) 35·4 (1) 35·2	34·4 34·2 (2) 34·6	34·4 34·8 33·5	22·7 21·5
,, *C*	35·2 (1) 33·0 (1) 33·0	35·6 (1) 37·0 33·4	36·2 (2) 37·0 34·6 (1)	34·8 (1) 34·4 (1) 35·2	33·8 33·6 (1) 34·6	21·4
,, *D*	35·0 37·0 37·4 (1)	39·0 (1) 37·4 36·6	37·2 (1) 35·4 37·0 (1)	36·2 35·4 37·2	36·0 37·2 35·2	22·7

(Each trial 40 cards; number of errors is shown in brackets. The figures for prearranged cards are taken from Experiment II.)

A number of subjects were tested on tasks of various ratios in order further to confirm the suitability of Equation (2). It soon became apparent that while the linearity of regressions was good at low and moderate values, too much time was being taken over the difficult tasks by most subjects.

Experiment II. Signals of Various Ratios and Constant Magnitude.

Cards of type (ii) were used; the pairs chosen being 1, 10; 5, 10; 8, 12; 9, 12; 8, 10; and 10, 12. In preliminary trials it had been found that large numbers gave more consistent results than small ones, probably because counting was out of the question. The effective stimulus seems to be the amount of black on the card, which is, of course, proportional to the number of spots. The subjects were the same as in Experiment I., and the experimental trials for this experiment therefore were preceded by about 30 practice ones.

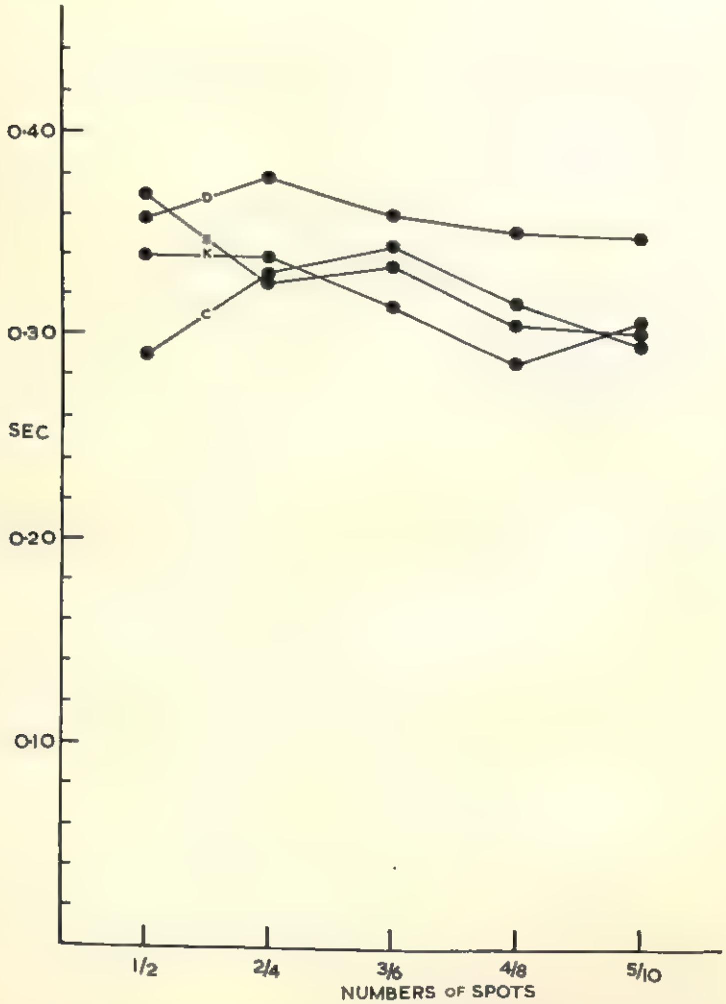

FIG. 2. Card-sorting by number of spots; paired signals of constant ratio and various magnitudes. Each point is the mean choice-time per card averaged over 3 trials of 40 cards each. (Expt. I.)

The results (Table III) are shown in Figure 3 plotted against values of Equation (1). Although the linearity of regression at low values is good, the curvature at the top could not be ignored. At the same time certain theoretical difficulties with Equation (1)

became apparent. The question of extending the theory to multi-choice and multi-dimensional signals was under consideration, and for this the formula had formal disadvantages, most importantly that it is not symmetrical in the x's. Further, the probability distributions assumed by Wiener in deriving his result did not seem to be the correct ones here. Further search was therefore indicated.

TABLE III

TIMES FOR SORTING BY NUMBER OF SPOTS (EACH TRIAL 40 CARDS)

	1/10	5/10	8/12	9/12	8/10	10/12	*Pre-arranged*
Subject *K*	31·4 (1) 31·8	30·4 31·6 32·4 (1)	34·2 33·2 32·8	37·0 (1) 35·6 (1) 37·4	39·8 (1) 39·2 (3) 38·6 (2)	42·4 (1) 40·6 (1) 43·8 (4)	21·4 22·7
,, *S*	28·0 (1) 27·4 28·0	31·1 30·0 (1) 30·2	31·4 29·2 31·6	35·0 35·4 (1) 36·4	36·2 39·0 37·2 (1)	38·0 (3) 40·6 (3)	22·7 21·6
,, *C*	29·0 (1) 29·0 29·6	32·2 33·4 (2) 33·4	34·2 34·4 34·4 (1)	36·2 34·4 36·2 (2)	39·6 (2) 40·8 (4) 40·4 (2)	45·6 (2) 44·0 (2)	21·4
,, *D*	29·4 29·2 28·2	33·0 32·0 31·6	34·0 (1) 35·0 35·8 (1)	34·0 39·4 (3) 36·6 (2)	40·0 (2) 39·4 (2) 41·2 (2)	45·2 (5) 42·4 (5)	22·7
Entropy ..	1 bit	1	1	1	1	1	0
Confusion ..	0·30	1·00	1·71	2·42	3·08	3·82	
Measurement information	0·15	1·00	1·55	2·00	2·32	2·60	
Statistical measure ..	0·38	2·25	6·25	12·25	20·25	27·50	

(Figure in brackets are number of misplaced cards.)

(*b*) *Statistical Operations carried out by the Brain.*

When a signal of magnitude x_q, known "a priori" to be one of two alternatives x_1, x_2, is presented to a human sense-organ, the process by which the brain decides which of the two it is may well be akin to the usual statistical method for testing between two hypotheses given a small sample. Let us suppose that the brain, in effect, makes a series of successive independent observations of x_q and averages them. Further let the observed values be distributed normally with mean x_q and coefficient of variation V $(= \sigma_q/x_q)$ a constant (the assumption of a constant standard deviation leads to obviously wrong results), and let each observation take δt seconds. The standard deviation of the mean of n observations will be given by—

$$\sigma_m = V . x_q/\sqrt{n} \quad \ldots \quad (2)$$

The probability of error will be small if decision is postponed until σ_m is, say, 1/3 of the actual difference $|x_1 - x_2|$. We can then write

$$| x_1 - x_2 | = 3\sigma_m = 3V . x_q/\sqrt{n}$$

and since n observations take $n.\delta t$ seconds, the time required is

$$t_{\mathrm{d}} = n.\delta t = K\left(\frac{x_{\mathrm{q}}}{x_1 - x_2}\right)^2 \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad (3)$$

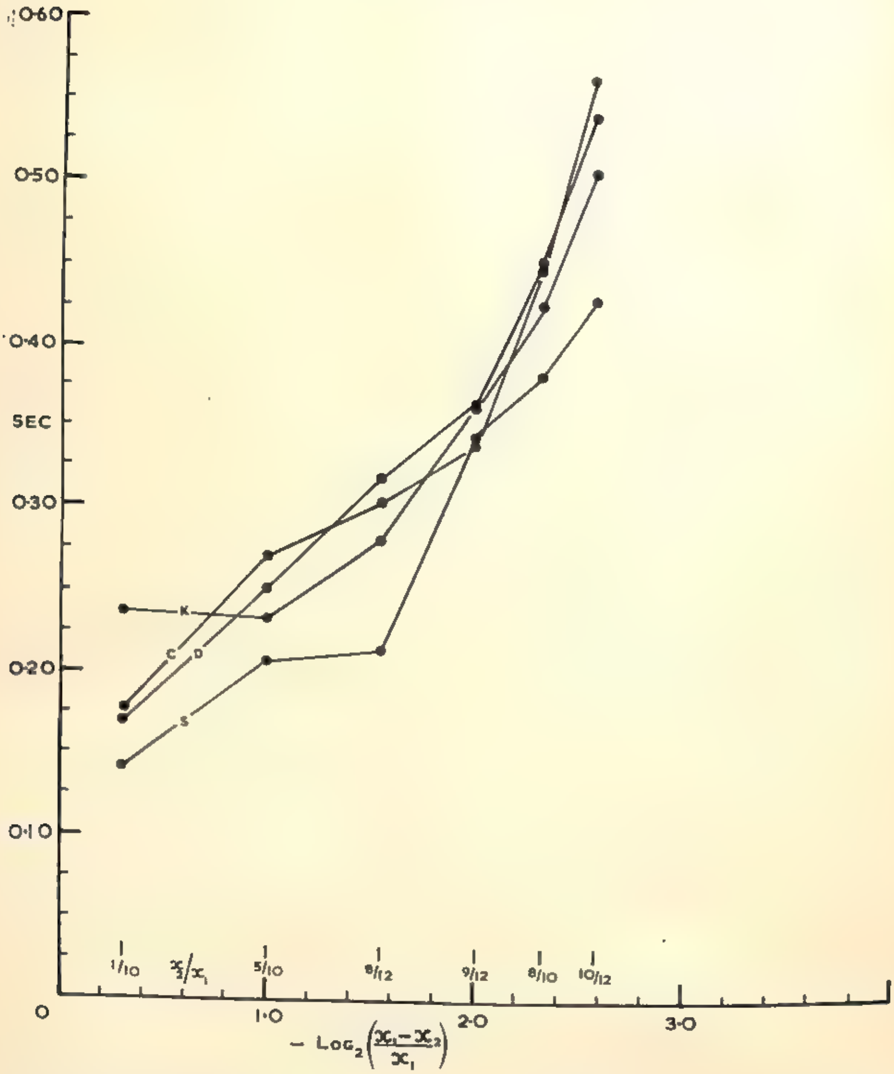

FIG. 3. Card-sorting by number of spots; paired signals of various ratios and constant magnitude. Choice-time per card plotted against values of the Information function given in Equation (1). (Expt. II.)

According to this formula the discrimination-time should differ between the two alternatives, and it should also depend on the absolute value. In order to avoid these two undesirable features we can substitute for x_{q} the arithmetic mean of its two values x_1 and x_2, obtaining for the time taken

$$t_{\mathrm{d}} = K'\left(\frac{x_1 + x_2}{x_1 - x_2}\right)^2 \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad (4)$$

This hypothesis was tested by replotting the data of Experiment II as shown in Fig. 4, but did not fit any better than the previous hypothesis, though the non-linearity appeared to be at the lower end this time.

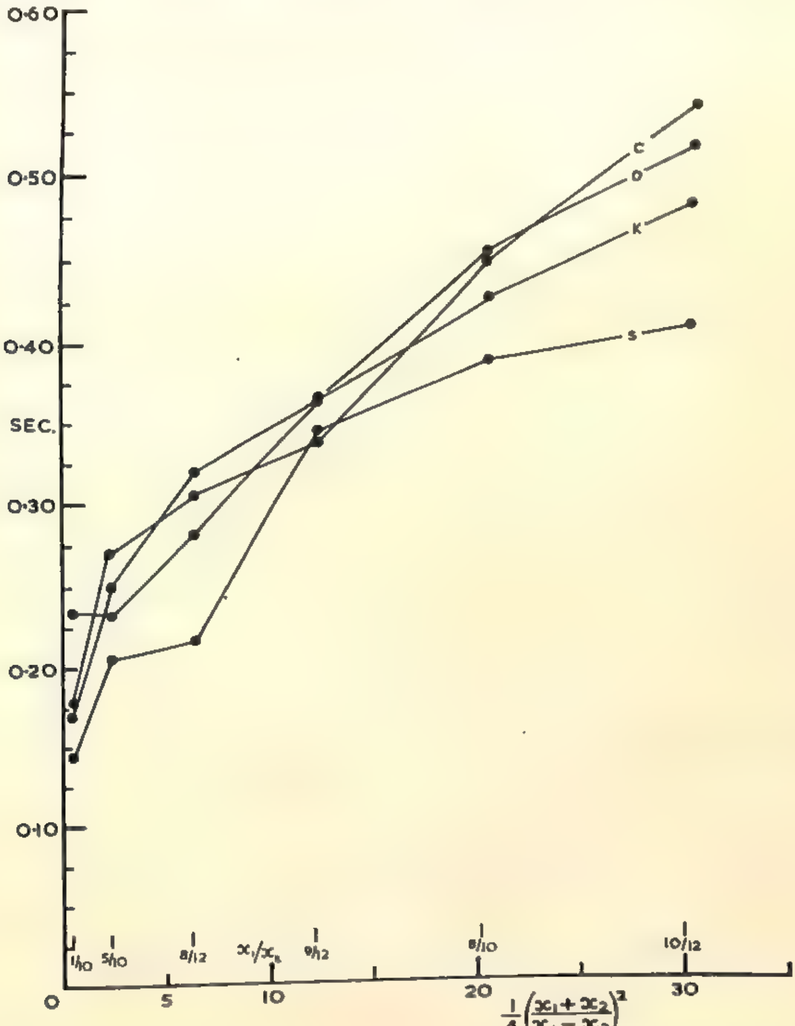

FIG. 4. Card-sorting by number of spots; paired signals of various ratios and constant magnitude. Choice-time per card plotted against values of the Statistical function given in Equation (4). (Expt. II.)

(c) *Confusion between Signals.*

When the second approach outlined above broke down the problem was restated in geometrical form. A new function, the Confusion-function, emerged from this line of thought. It appeared to be similar to the Information function, but to be both algebraically more manageable and to give a better fit to the data. The argument by which it was obtained runs as follows.

Consider two signals S_1 and S_2 which can be distinguished by the fact that they possess different values x_1 and x_2 of a physically measurable characteristic x. (In Lemmon's experiment, for example, x would be number of lights or total light

intensity). We require a function of the two variables x_1, x_2 which will increase monotonically as they become more difficult to distinguish. We can consider S_1, S_2 as points in a space of one dimension located at distances x_1, x_2 from the origin. The ease of distinguishing between them might then be expected to depend on the "distance" between them. The simplest possibility, that the distance is $|x_1 - x_2|$ will not do since the ease would then depend on the absolute difference between the signals, and we know from experiment that it depends rather on their *ratio*. In order to make the distance dependent on ratio rather than on absolute difference, we can take logarithms and measure in the space of log x. The "distance" then becomes

$$D(S_1, S_2) = |\log x_1 - \log x_2| = |\log (x_1/x_2)| \quad \ldots \quad (5)$$

This "D-function" gives the *ease* of discrimination for S_1 and S_2. We require on the contrary a function giving the *difficulty*; we therefore take the reciprocal and obtain—

$$C(S_1, S_2) = 1/D(S_1, S_2) = 1/|\log x_1 - \log x_2| \quad \ldots \quad (6)$$

$$= 1/\left|\log.\left(\frac{x_1}{x_2}\right)\right| \quad \ldots \quad (7)$$

These expressions are symmetrical in the x's, which we can express by writing

$$C(S_1, S_2) = C(S_2, S_1).$$

If more than one dimension is being used in discrimination, the distance D can be calculated in the usual geometrical way by taking the square root of the sum of squares of the separate distances. The function of Equation (6) has been named a "Confusion-function" since it measures the tendency to confusion between S_1 and S_2. The choice of the base of logarithms is at our disposal; it sets the scale. The base 2 has been used here, since the unit is then simple, being the confusion between two signals one half the size of the other. Values of the function are given in Table IV for integral x_1, x_2 from 1 to 10.

TABLE IV

VALUES OF THE CONFUSION-FUNCTION FOR INTEGRAL ARGUMENTS

x_2/x_1	1	2	3	4	5	6	7	8	9	10
1	*	1·00	0·63	0·50	0·43	0·39	0·36	0·33	0·31	0·30
2		*	1·71	1·00	0·76	0·63	0·55	0·50	0·46	0·43
3			*	2·41	1·36	1·00	0·82	0·71	0·63	0·58
4				*	3·13	1·71	1·25	1·00	0·86	0·76
5					*	3·82	2·07	1·48	1·18	1·00
6						*	4·51	2·41	1·71	1·36
7							*	5·18	2·76	1·94
8								*	5·88	3·13
9									*	6·58

The results of Henmon for pairs of lines and pairs of tones (produced by loaded tuning-forks) are shown plotted against the corresponding values of the Confusion-function (7) in Figure 5. It will be seen that the data do not depart markedly from straight lines, and that the curves for visual and auditory tasks fit together surprisingly well in the case of subjects "S" and "W." Subject "H" shows a rather different picture, his times are surprisingly low for so difficult a discrimination.

The data of Experiment II were again replotted (Fig. 6) and this time there was no evidence of departure from linearity. Analysis of variance showed that the points did not depart significantly from the calculated regression-line. The conclusion was drawn that the Confusion-function is a good measure of discrimination difficulty in the case of visual judgments of number.

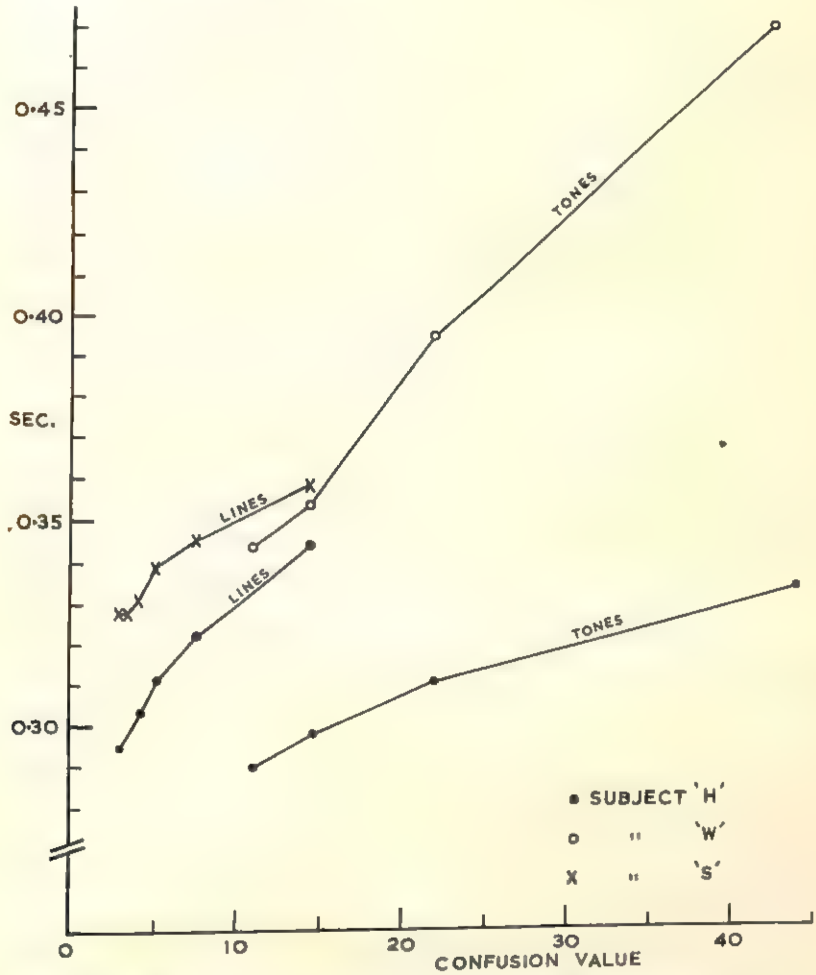

FIG. 5. Results of Henmon (1906) replotted against values of the Confusion-function given in Equation (7).

In this and similar experiments on visual number the threshold (for judgment without counting) lies at or about 5 C-units. Above this point no reliable results are obtained because either the subject makes many errors or is forced to count the spots instead of making an immediate decision.

Experiment III. Discrimination of Weights by Lifting.

The outcome of the previous experiments substantially confirmed our third hypothesis with a visual display. If, as was hoped, the confusion function is of general application, one should be able to interpret results obtained in other sensory modes by its use.

An experiment was devised to test whether the same relation would hold good in weight judging. Two groups of 8 small aluminium cans were loaded with two different weights, mixed and placed in a row on the table in front of a seated subject. At the word "go" he lifted first a sample Light and a sample Heavy can, then worked down the row with his right hand, weighing each can between his finger and thumb, and moving "Heavy" cans away from him, "Light" ones towards him, at top speed. The whole performance was timed on a stop-watch, and took 10 to 20 seconds. The weights

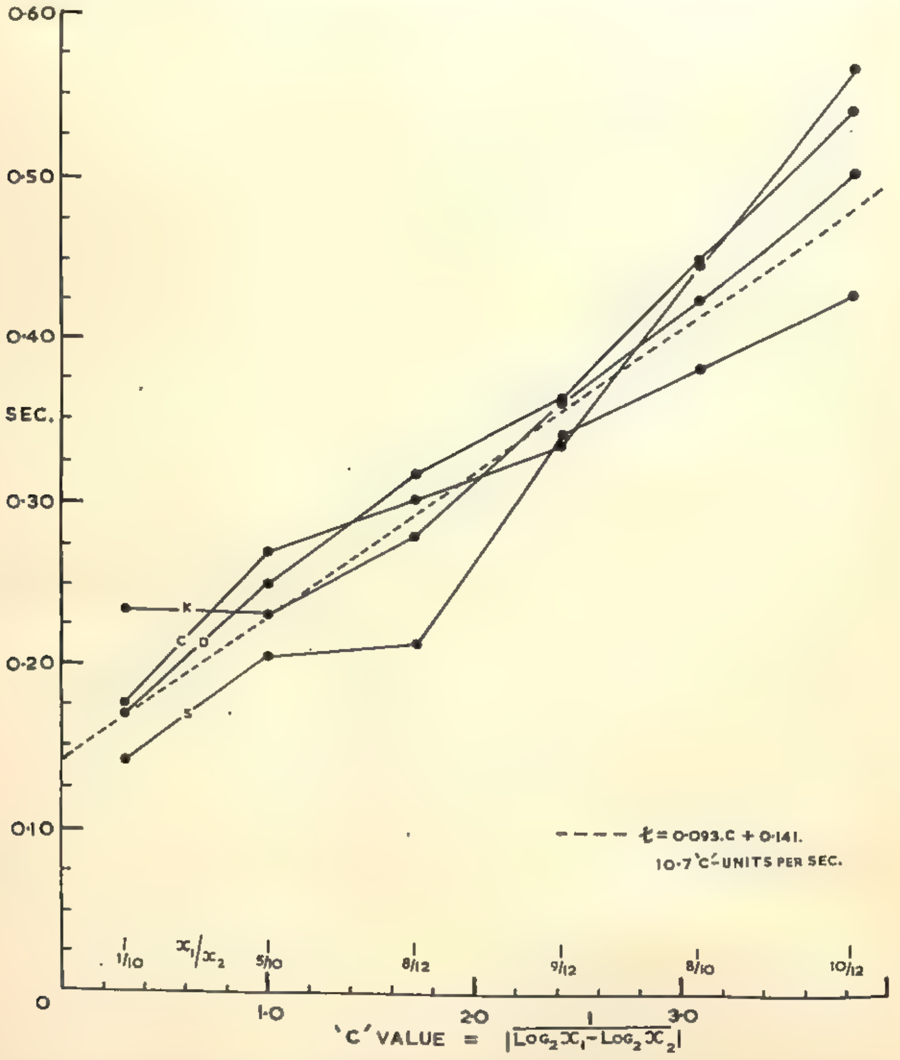

FIG. 6. Card-sorting by number of spots; paired signals of various ratios and constant magnitude. Choice-time per card plotted against values of the Confusion-function given in Equation (7). (Expt. II.)

were varied systematically in order to provide tasks with various Confusion-values according to Table IV. The experiment is a modified form of the classical experiment on the differential threshold for judgment of weight. Easy judgments are made, and the time taken is measured instead of proportion correctly judged. A "standard" is found to be unnecessary, its place being taken by the rapidly presented sequence itself,

which provides its own standard. The manual dexterity of the subject must be good for consistent results. Care was taken to avoid providing cues other than weight. Times for one subject are shown in Figure 7. It will be seen that a straight line of slope 0·23 sec. per confusion-unit fits well.

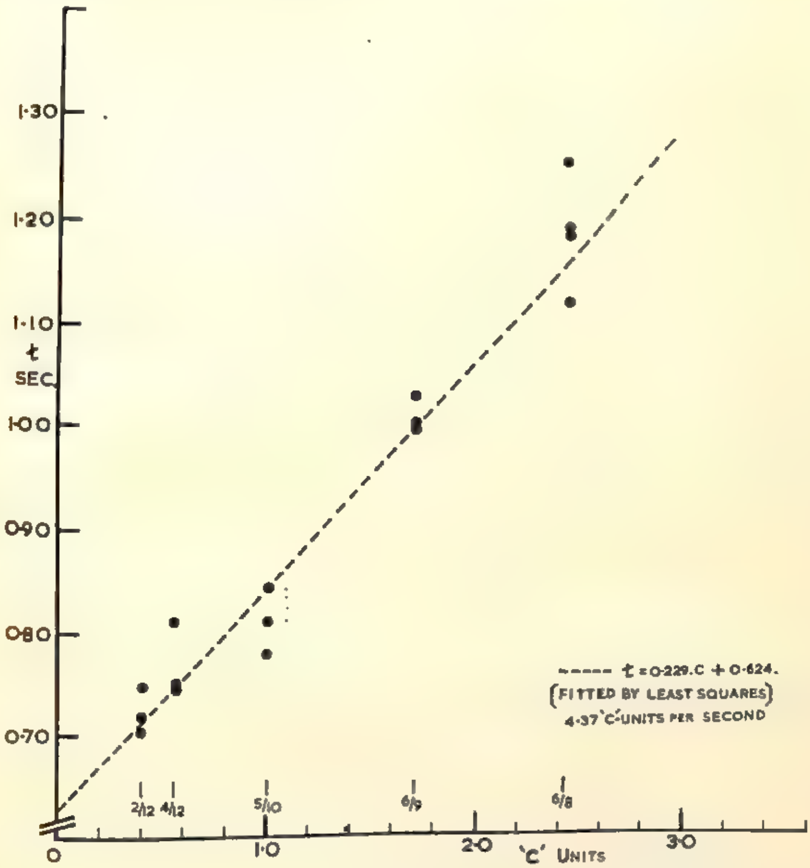

FIG. 7. Sorting by weight. Time taken to sort paired weights of various ratios by hand plotted against values of the Confusion-function given in Equation (7). Each point represents a single trial of 16 items. (Expt. III.)

The results of Experiments I to III all strongly support our third formal argument. The Confusion-function of Equation (6) appears to be a good measure for the difficulty of discrimination of visual, auditory and kinaesthetic signals over a range of conditions. We may expect that the same equation would yield good results for one-dimensional signals in other sensory modes.

IV

Confusion between Signals in a Multi-Choice Task

Since experimental support for the Confusion-function as applied to two-choice discriminations was encouraging, the next step was to extend the treatment to multi-choice ones. The conclusions so far reached in this field are not as clear as could have been hoped, but will be reported in brief. Some extension to the theory is first required.

Let us consider a sequence of signals in which n different alternatives $S_1, S_2 \ldots\ldots S_i \ldots S_n$ may occur; let them occur independently in the sequence with equal frequencies; further let each be recognizable only by its measure in a single dimension, the values being $x_1, x_2 \ldots x_i \ldots x_n$. We wish to measure the Confusion for the set of n; that is, the average difficulty of deciding which of the Ss a given unknown Q is. In the two-signal case, the measure we arrived at was that of the Equation (6); in order to extend it to the more general case we must make some assumption about how confusion-values combine. The confusion on a particular signal might be governed only by its "nearest" neighbours; or all the alternatives might play a part according to some rules of combination. We consider here only the second hypothesis, since it is the more general, and we suppose confusions to be additive. The operation we may imagine a subject's brain to perform is this. When a signal Q arrives, it is tentatively identified as one, S_i, which will in most cases be the correct one. The signal or its copy is then compared successively with every other possibility, S_j, and the answer "different" is obtained for each after a time proportional to the confusion C_{ij} between the two. Only after all these answers have been obtained will Q have been positively identified as S_i. Since the time taken in each comparison is proportional to C_{ij}, the time to identify S_i will be

$$t_i = A . \sum_{j=i}^{n} C_{ij} = A . C\,(i) \quad \ldots \quad \ldots \quad (i \neq j)$$

Since S_i occurs $1/n$ times per signal, its contribution to the time-per-signal will be $A . C_i/n$ and the average time over the whole sequence will be (we consider at present only the case of equiprobable signals):

$$t = \frac{A}{n} \sum_{i} C\,(i) \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad (8)$$

Remembering that $C_{ij} = C_{ji}$, the expression (8) can be simplified to yield:

$$t = A . C(n) = A . \frac{2}{n} \text{ (Confusion summed over all pairs)} \quad \ldots \quad \ldots \quad (9)$$

As an example we may take a three-choice sorting task in which cards of 3, 5, 7 dots occur with equal frequency

$$\begin{aligned} C\,(3) &= C\,(3, 5) + C\,(3, 7) = (1{\cdot}36 + 0{\cdot}82) = 2{\cdot}18 \\ C\,(5) &= C\,(5, 3) + C\,(5, 7) = (1{\cdot}36 + 2{\cdot}07) = 3.43 \\ C\,(7) &= C\,(7, 3) + C\,(7, 5) = (0{\cdot}82 + 2{\cdot}07) = \underline{2{\cdot}89} \\ & \qquad\qquad\qquad\qquad\qquad\qquad\qquad\qquad\; 8{\cdot}50 \end{aligned}$$

and averaging these

$$C\,(3, 5, 7) = C\,(3)/3 + C\,(5)/3 + C\,(7)/3 = 2/3 . (1{\cdot}36 + 0{\cdot}82 + 2{\cdot}07) = 2{\cdot}83.$$

Experiment IV.

Cards of type (ii) were put together in various combinations of up to five numbers; a wide range of discrimination difficulties was selected. The experimental procedure remained as before except that the pack was larger. Values of "confusion" were calculated for each combination by the use of Equation (9) and Table IV. No important difference from the two-choice task was observed in the way subjects approached the new more complicated sorting task, nor in their performance at it. Six young adult subjects were tested. The results are given in Table V, and one subject's times are plotted in Fig. 8. It is hardly fair to claim that the data fit our hypothesis perfectly; yet the points for different numbers of choices from 2 to 5 do not diverge noticeably and on the whole a straight regression-line seems the best choice. Two factors may

account for the few "wild" points; practice effect which could not be entirely balanced out, and the use of "blank" as one of the signals. The latter should have zero confusion with all other alternatives, but times are higher than would be expected on this basis. We tentatively conclude that the Confusion-function fits multi-choice tasks of up to 5 alternatives of a one-dimensional signal.

TABLE V

MULTI-CHOICE SORTING BY NUMBER OF SPOTS. VARIOUS DEGREES OF DISCRIMINABILITY

N	*H*	*C*	B	R	L	S	D	G	*Task*
2	1.00	0·76					0·845		2, 5
		1·00	0·820	0·813	0·875		0·819	0·787	1, 2, etc.
		1·71			(0·920)	0·809		0·822	2, 3, etc.
		2·42		0·918		0·932			3, 4, etc.
		3·08	0·962	1·011	1·085	0·978	1·127	0·925	4, 5, etc.
		3·82	1·164		1·125		1·300		5, 6
		4.50		1·133	(1·825)	(1·205)	1·825	1·170	6, 7
3	1·58	0·67			0·914				0, 1, 2
		1·67	0·935	0·918	0·968	0·856	0·980	0·877	1, 2, 4
		3·42	1·078	1·020	1·135		1·088	0·926	2, 3, 4
		4·85				1·017			3, 4, 5
		5·74	1·236						4, 5, 6
4	2·00	2·17	0·998	0·982		0·909	1·100		1, 2, 4, 8
		4·23			1·218				2, 3, 4, 6
		4·63	1·161	1·087			1·181	1·050	3, 4, 6, 8
		6·75	1·254						4, 5, 6, 8
		6·90				1·077			3, 4, 5, 6
5	2·31	1·73		1·048	1·121			1.001	0, 1, 2, 4, 8
		5.24	1·184	1·150	1·261			1·106	2, 3, 4, 6, 8
		6·01					1·470		3, 4, 6, 8, 10
		7.89	1·480						4, 5, 6, 8, 10
		8·95				1·540			3, 4, 5, 6, 7
2	1·00	0·00	0·735	0·783	0·845	0·772	0·815	0·755	0, 1, etc.
MT	0·00	0·00	0·474	0·583	0·590	0·583	0·587	0·600	

(N = number of choices; H = Entropy per card; C = Confusion value. Each entry is a time per card averaged over 120 or more cards. Figures in the task column are numbers of spots.)

V

GENERAL DISCUSSION

(1) *Relation of these results to Information Theory findings.*

The reader will have noticed that in Experiment IV the information-per-signal was greater for some conditions than for others; a two-choice task carries 1·00 bits of selective information per card and a five-choice 2·31 bits. If an information-rate of 5 bits per second be assumed, the corresponding time-difference should be 0·26 second. No account of this has been taken in calculating the confusion values for each task which yet fit the data well. In other words, if discriminability is held constant, no increase of time with information-content occurs, within the limits of accuracy of this experiment.

In theory discriminability (or confusion) and information content should be inter-related at least for signals in one dimension when an upper limit is set to signal size.

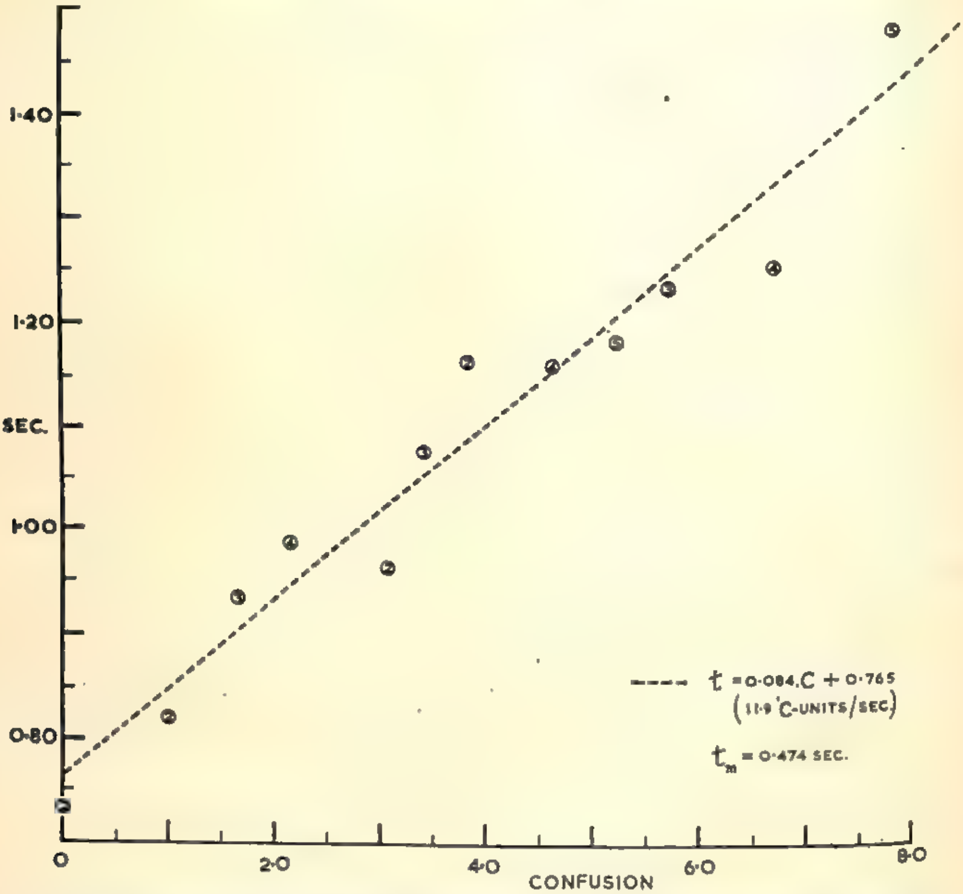

FIG. 8. Card-sorting by number of spots; 2, 3, 4 and 5-choice tasks of various degrees of discriminability. Time per card is plotted against values of the Confusion-function given in Equation (9). Results of Subject B only are shown. (Expt. IV.)

For if information content is to be increased, more signals must be fitted or "coded" into the signal space; they must then be closer together and so more confused. In previous experiments on Information Theory signals were certainly not one-dimensional, and the discriminability was not controlled; herein may lie the key to our inconsistency. It may be that simple dependence of choice-time on information content is the limiting case which occurs when the number of dimensions is large. Further work is necessary to resolve the difficulty.

(2) *Relation of results to the Weber-Fechner Law.*

Weber's law states, in brief, that the just noticeable difference between sensory stimuli is a constant fraction of their absolute magnitude (Weber, 1834).

$$\frac{\Delta s}{s} = \text{a constant (the Weber Fraction)}$$

where Δs = a just noticeable change

s = physical stimulus magnitude.

This equation was integrated by Fechner and stated as a law relating stimulus and sensation (Fechner, 1860).

$$S = K \log\left(\frac{s}{\beta}\right) \ldots\ldots (10)$$

S = sensation magnitude
K = a constant
β = the threshold stimulus.

He further explored the implications in some detail, developing among other expressions an "Unterschiedsformel" (1860, p. 89), which is similar to our Distance-function of Equation (5). Since Fechner put forward his law there has been much debate over the validity of measuring sensation in this way, but none about the mathematical form of the law. The idea that a scale of sensation can be set up is perhaps reasonable on the evidence of introspection, but critics object that unwarrantable assumptions underlie the procedure by which the law is arrived at (see, for example, Henmon, 1906, pp. 5–11). Fechner's formulation rests implicitly on the idea that a just-noticeable difference is the unit of sensation; an alternative (Method of Equal-appearing Intervals) requires an observer to match two pairs of stimuli for equal difference of sensation. Neither of these judgments is introspectively definite enough to support a positive theory. But the logarithmic transformation of stimulus magnitude has long been recognized to be important in some sense (according to Fechner it was first suggested by D. Bernoulli in 1738 in a paper entitled "Specimen Theoriæ novæ de Mensura Sortis"). Our results perhaps provide the basis for a more operational interpretation of Fechner's law in the following way.

One would expect to find it easier to distinguish between very different sensations than between ones nearly the same. If now we suppose this difference of *ease* to be manifested in an inversely proportional difference of *time*, we may write for the time taken to distinguish sensations S_1 and S_2:

$$t\,(1, 2) = \frac{k}{S_1 - S_2}$$

and substituting for S_1, S_2 from Equation (10) (Fechner's law) we obtain

$$t\,(1, 2) = \frac{k}{\log s_1/\beta - \log s_2/\beta}$$

Apart from the threshold constant, β, this relation is identical with the Confusion-function which we have obtained by another route and verified experimentally without any consideration of thresholds or subjective matching. Our Confusion-function may, therefore, be regarded as a new version of the Weber-Fechner law extending it to discrimination in general and giving it an operational interpretation in terms of response-time. The precise relation of our results to threshold phenomena has not been worked out; but it is at least possible that existence of a threshold may be accounted for by supposing the time over which discrimination can improve to be limited. In Experiment II the time-limit would be about $\frac{1}{2}$ second.

(3) *Perception and recognition.*

The process we have been studying can as well be called "recognition" as "discrimination"; and perception has been sufficiently shown to be primarily a process of recognition of previously established and expected signals. The difficulty of recognition, or likelihood of confusion, can be calculated from a knowledge of the appropriate physical dimensions of the display, in the simple cases we have studied. In principle, the same method should be applicable to the much more complicated recognitions or perceptions of everyday life, in the same way that the physical laws of motion could be applied to a game of football.

(4) *Span of apprehension.*

Our experimental display is similar in important respects to that used in the classical type of experiment on span of apprehension. In both the subject is required to decide how many spots there are in a random group, given only a short exposure. The crucial difference is that in the card-sorting only a small set of numbers may occur (e.g. sorting fours from threes) whereas in the more usual tachistoscopic presentation any number up to about fourteen may occur. Our results show that the

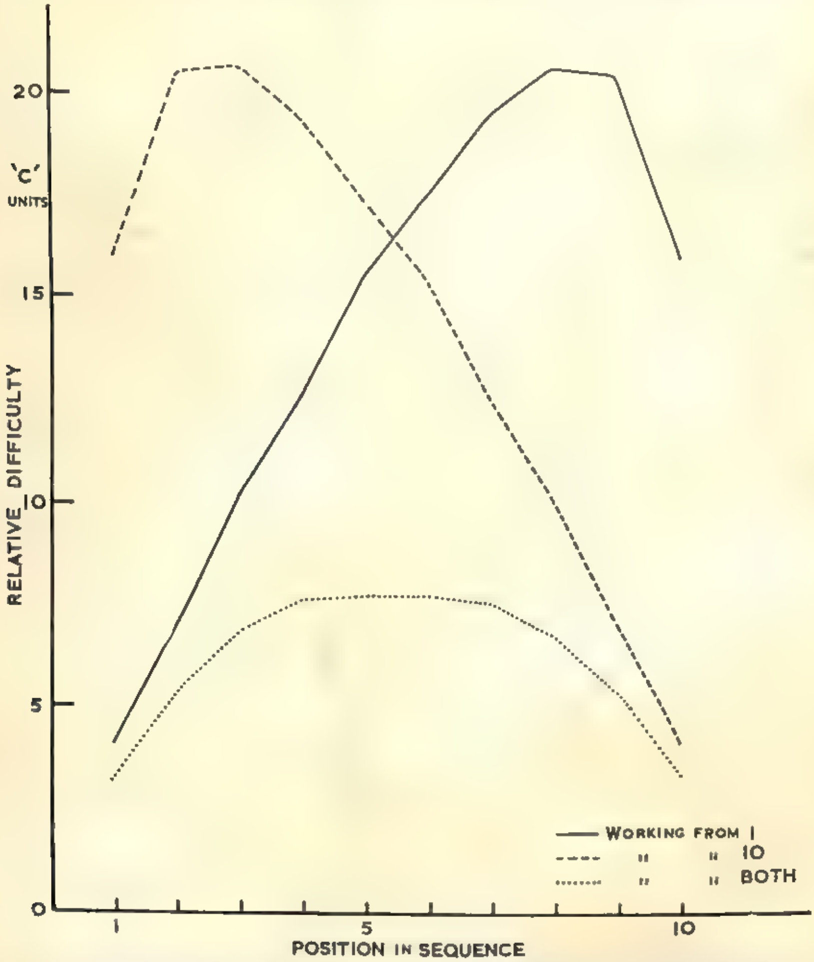

FIG. 9. A theoretical curve of the Relative Difficulty of locating Positions in a Sequence of 10 items, calculated from the Confusion-function of Equation (7).

difficulty of distinguishing a particular number of spots from another depends on their ratio. This ratio when calculated for pairs of adjacent digits, diminishes successively and reaches threshold at about six or seven. Thus 5 and 6 can be sorted without counting and sometimes 6 and 7, but hardly ever 7 and 8. If the actual number is 7 the subject will be unable to say that it was not 8 and will make many errors. But if asked to sort 8 from 10 he can do it easily. The existence of the so-called Span of Apprehension seems therefore to be due to the fact that the ratio between adjacent digits becomes smaller as the digits become larger. The idea that the mind can only

grasp a small number of objects at once, remains quite unsupported by the evidence, if indeed it has any meaning at all.

(5) *Memory and Position of symbols in a sequence.*

It may be possible to explain some results in the learning of sequential material by the use of our Confusion-function. Let us consider a sequence of symbols, say nonsense syllables, which is to be learned; position in the sequence can be regarded as a one-dimensional variable, and each symbol will have one value of this variable associated with it. The likelihood of confusing the nth position with the $(n+1)th$ will be given by $1/\log\left(\frac{n+1}{n}\right)$ which increases steeply with n (table IV). If the sequence is endless, the difficulty of placing increases indefinitely, but if it has an end, symbols after the middle are easier to deal with by working from the end backwards. A plot of difficulty against position on this basis gives a symmetrical curve (Figure 9), with which observed error-scores may be compared.

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